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THE RELATION OF TIME OF LIFE TO BEHAVIORAL CONSEQUENCE IN BRAIN DAMAGE: I. THE PERFORMANCE OF BRAIN-INJURED ADULTS ON THE MARBLE BOARD TEST

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INTRODUCTION

An understanding of the consequences for behavior produced by cerebral damage involves a consideration of at least four variables: locus of injury, extent of neurological damage, type of lesion introduced, and the point in time in the life history of the individual at which the damage has been sustained. Considerable attention has been devoted to the problems of locus and extent of damage, whereas relatively little work has been done on either the characteristics of the lesion in a functional sense (2) or upon the differential consequences which may ensue from the occurrence of even identical neurologic insults at different life stages.

Attempts at understanding the significance of the point in time of cerebral damage have largely been limited to investigations at the animal level (4). These studies have indicated that the consequences of anatomically similar lesions are quite different in accordance with their experimental introduction in infancy or in maturity. Hebb (4), in a most stimulating analysis of this problem, has also provided evidence that suggests that at the human level, too, the time in life at which a cerebral insult is sustained has a significant bearing upon the behavioral consequences of the damage. As yet, however, no systematic effort has been made to analyze comparatively the effects upon varied defined behavioral indicators of early as opposed to late brain damage.

It is abundantly clear that this neglect, at least at the human level, has stemmed from the difficulties which attend such comparative investigation. The number of different variables to be controlled is very considerable and the populations required for fully controlled exploration of the problem are in no instance readily assembled. Nevertheless, it is perhaps possible to begin work on the problem under circumstances that do not permit of entirely adequate controls and to explore aspects of the problem in a phenomenologic way in advance of an optimally structured inquiry. Such preliminary efforts can serve at least two functions. In the first place, they may bring the existence of the problem to the attention of other investigators and perhaps function to stimulate further investigations. In the second place, such preliminary investigations can serve to define more clearly the types of behavioral alteration that it would be most profitable to explore in subsequent studies.

A potentially useful approach to the problem stems from the attempts at the systematic investigation of certain perceptual and cognitive functions made by Werner *et al.* (8, 9) in children. In these studies an attempt was made to compare perceptions and cognitions of children with congenital cerebral damage with those of intellectually matched children in whom no evidence of anatomical injury to the nervous system could be ascertained.

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One way of approaching our problem is through what could be called a method of *pseudo-comparison*. It is possible to utilize the same situations as those which were employed by Werner and to apply them to a population of adults who sustained cerebral damage in later maturity. One can control for the effects of aging, and in general for the factors of maturing and learning, by including a control group of adults who are matched for age and social experience with the brain-damaged population. Under these circumstances, it is possible to compare the adult brain-damaged performances with those of the controls and then to relate the *differences* in performances obtained in such comparisons with the *differences* which Werner found to exist between his brain-damaged and non-brain-damaged groups of children. Such a comparison would permit us to explore the character of the deviations from normal functioning which are introduced at different age levels by the fact of cerebral damage. It is clear, therefore, that the method does not involve a direct comparison of brain-injured adults with brain-injured children. Rather it is a comparison of the differential effects upon the performance of specific tasks of brain damage sustained at different points in time in the life history of the individual.

In the present paper the effects of cerebral damage upon the performance of one of these tasks, the marble board, a test of visual form reconstruction, is analyzed. In subsequent reports the results of other perceptual and congitive studies will be presented.

METHOD

The subjects of this study were patients on the rehabilitation wards of a large municipal chronic disease hospital. Forty adult patients were used in this study. Twenty of the patients were brain-damaged as a result of cerebral vascular accidents (CVA) and twenty of the patients were non-brain-damaged, orthopedically handicapped individuals. The subjects were selected on a random basis from a master file which contained the records of all patients currently receiving treatment on the rehabilitation service. The brain-damaged patients used in the study were between 50 and 70 years at the time of damage. In all cases at least six months had elapsed since the original injury, and no severe aphasia, no hemianopsia, and no uncorrected visual or hearing defects were present. The non-brain-damaged patients were matched for age and social background. One small discrepancy between the two groups was in the sex distribution. There were 8 men and 12 women in the brain-damaged group as compared with 14 men and 6 women in the control group. Because of the inequality of males and females in the two groups, the data were analyzed with respect to sex. This analysis revealed no significant differences to exist between the sexes with respect to mode of problem solution or number of correct designs. It was, therefore, decided that the sex ratio inequality in the groups made no significant contribution to the differences obtained.

The marble board test used consisted of two boards, 11 inches square, which contained 10 rows of 10 holes each (100 holes per board). The six designs that were used are presented in Figure 1. All the designs, with the exception of Design No. 5, were constructed with black marbles. Design No. 5 involved the use of red marbles in addition to the black.

The examiner constructed the design on one of the boards out of sight of the subject. The subject was shown the completed model and was asked to copy the examiner's design while it was in view. The moves of the subject in copying the design were recorded by the examiner. In this way, a complete record was available of the subject's test behavior.

The following instructions were given to the subjects:

"Here is a design made up of marbles. I would like you to make one just like it on

this board, which is exactly like mine. Take as much time as you need."

The examiner recorded the moves made and, when the subject was unsuccessful in constructing the demonstration pattern, the method of solution was explained. The examiner determined that the subject understood the task before proceeding with the test designs.

When a design was not copied correctly, the examiner ascertained if the subject was finished when he appeared to stop working. There was not time limit applied. In addition, the examiner routinely asked, "Is your design just like mine?" If the subject noticed a difference and could then attempt to correct it, he was allowed to do so.

In order to evaluate the subjects' behavior on the marble board task, it was necessary to codify the findings. Categories for analysis were established identical with those used by Werner and Strauss to facilitate comparison with the analysis of behavioral alteration observed by them in their studies of children with brain damage sustained in early life (8). Certain additional categories were added in order to permit more detailed analysis of adult behavior *per se*. All performances were analyzed to determine:

1. Accuracy of design duplication.
2. Errors and direction of errors:
   (a) jumps or discontinuity of moves;
   (b) corrections which may involve either additional unnecessary movements or corrections of the whole pattern;
   (c) line dissection: the placement of the marbles in such a way that a continuous line is not completed as a whole but is done in two parts starting at or near the midpoint. In such cases, the line is first constructed to the left or right of the starting point and then completed from the same starting point by construction in the opposite direction;
   (d) dissection of form: the separation of the model into discontinuous sub-forms.
3. General method of reconstruction: a characterization of the sequential procedure as constructive, global, linear and incoherent or unsystematic.

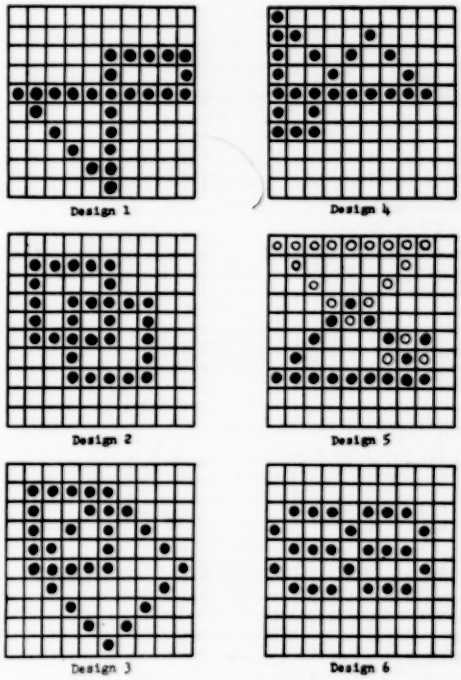

FIG. 1. Marble Board Designs.

All scoring methods were subjected to duplicated blind analysis and exhibited repeat scoring reliabilities with a rank-order coefficient of correlation of .93.[2]

DISCUSSION OF RESULTS

The data presented in Tables 1 through 6 indicate that significant differences exist between the adult brain-damaged and control groups in terms of accuracy of response, methods of reconstructing designs, and characteristic types of errors made. All variables result in comparisons that are favorable to the control group.

[2] Spearman rank correlation coefficient corrected for tied ranks (6, p. 207).

TABLE 1

Correct Reproductions by Adult Brain-Damaged and Control Subjects

Group	No. of Designs Correct		t	p
	Mean	S.D.		
Brain-damaged (N = 20)	2.40	2.11	3.29	<0.01
Control (N = 20)	4.45	1.82		

TABLE 2

Subjects Making Correct Design Reproductions

Accuracy Index	Category	Group		Chi-Square*	p
		Brain-Damaged	Control		
No. correct per subject: those with 0–3 designs correct vs. those with 4–6 designs correct	0–3	12	4	5.10	<0.02
	4–6	8	16		
No. correct per subject: those with 0–4 designs correct vs. those with 5–6 designs correct	0–4	16	7	6.54	<0.01
	5–6	4	13		

* Corrected for continuity. For fourfold contingency tables, one degree of freedom is available.

When accuracy of performance is examined, it is found (Table 1) that the control group has a significantly greater number of correct responses and a fewer number of incorrect responses than does the brain-damaged group. Since accuracy in this case is a measure of the degree to which the subjects are able to reconstruct the design as a whole, it can be said that the brain-damaged individuals are in most instances (Table 2) unable to be successful in the reproduction of the total design pattern. The differences between groups are in all instances statistically significant.

Perhaps the most important way in which the brain-damaged group differs in its test behavior from the non-brain-damaged group is in the methods used by its members in reconstructing the presented designs. In Table 3 the method of reconstruction is analyzed in terms of three categories. The first of these is classified as a systematic pattern of response. This method refers to a mode of construction which showed an awareness of overall pattern. This awareness could take the form of an appreciation of the pattern as a whole or of the sub-forms of which the total was a construct. As may be seen from Table 3, the brain-damaged group differed from the non-brain-damaged subjects in making significantly fewer design reproductions in accordance with a systematic pattern of procedure.

No differences were found in the number of designs that were constructed by each group in terms of a systematic attention to background. This method involves the following of background columns and rows with no attention paid to the design itself.

When unsystematic responses are analyzed it is found that in 51 designs the brain-damaged subjects functioned incoherently whereas only 14 of the productions of the control patients could be classed as having been produced in an incoherent fashion. This difference is significant at the <.01 level of confidence.

TABLE 3

The Number of Designs Constructed by Different Methods

Method	Group				t	p*
	Brain-Damaged (N = 20)		Control (N = 20)			
	Mean	S.D.	Mean	S.D.		
No. of designs produced with systematic response to pattern	2.25	2.05	4.25	1.55	3.48	<0.01
No. of designs produced with systematic response to background	1.20	1.36	1.05	1.60	0.32	<0.40
No. of designs unsystematically produced	2.55	1.74	0.70	0.31	4.09	<0.01

* One-tailed test.

In Table 4 the percentage of unsystematic responses made to each of the designs by the brain-damaged and non-brain-damaged subjects is analyzed. It may be seen that the designs are not equally demanding in the degree to which they provoke unsystematic behavior in either group of subjects. In no single design, however, does the non-brain-damaged group exhibit a greater proportion of unsystematic responses.

The differences between the two groups is still further accentuated if one analyzes the number of subjects who made one or more unsystematic responses and those who made no unsystematic responses. In Table 5 it may be seen that the brain-damaged subjects were significantly more likely to make at least one unsystematic response ($p = .01$).

When the types of errors made by the two groups are analyzed (Table 6), it is found that in every instance the direction of difference favors the non-brain-damaged group. Only one of the differences compared, number of designs with corrections of a significant portion of a design, fails to achieve the .05 level of confidence. The most significant differentiator appears to be the difference which is obtained with regard to the number of designs which exhibit line dissection ($p = .01$).

In summary, Tables 1 through 6 reveal significant group differences in accuracy, method of design construction, and in types of error. Therefore, on a group level the reconstructive behavior measured by the marble board test does yield differences between the two groups of subjects and reflects a deficit resulting from brain damage over and above any alterations in adequacy of performance that may have occurred as a result of aging *per se*.

In order to compare the alterations induced by cerebral damage in the present adult group with the kinds of alteration in performance reported by Werner and Strauss for children, it was necessary to categorize the data presented by these investigators in terms of the variables utilized for analysis in the present report. Since no data on line trends are presented by these early investi-

TABLE 4

Percentage of Adult Subjects Responding Unsystematically To Each of the Designs

Design No.	Group	
	Brain-Damaged	Control
1	35%	5%
2	65	25
3	45	15
4	30	10
5	50	10
6	30	10

TABLE 5

Adult Brain-Damaged and Control Subjects Who Constructed One or More Designs By An Unsystematic Method

Group	No. of Subjects with One or More Unsystematic Productions	No. of Subjects with No Unsystematic Productions	Chi-Square*	p
Brain-damaged	17	3	5.38	<0.02
Control	9	11		

* Corrected for continuity. For a fourfold contingency table, one degree of freedom is available.

TABLE 6

Significance of Differences In Types Of Error Made By Adult Brain-Damaged and Control Subjects

Type of Error	Group				t	p*
	Brain-Damaged (N = 20)		Control (N = 20)			
	Mean	S.D.	Mean	S.D.		
No. of designs with line dissection	2.50	1.32	1.15	1.04	3.60	<0.01
No. of designs with jumps	2.25	1.29	1.50	1.10	1.97	<0.03
No. of designs with correction of a significant portion of a design	0.95	1.05	0.60	0.31	1.11	<0.13
No. of designs with corrections (additional moves)	3.2	1.91	2.15	1.79	1.79	<0.05

* One-tailed test.

TABLE 7

Differences in the Number of Design Methods and the Types of Error Made by Brain-Damaged Children and Adults

Type of Response	Percentages of Responses					
	Children*			Adults		
	Brain-Damaged (N = 11)	Control (N = 10)	Difference between B = D and Control	Brain-Damaged (N = 20)	Control (N = 20)	Difference between B = D and Control
Method:						
Incoherent†	83	7	+76	42.5	12	+30.5
Line trend	—	—	—	20	17.5	+ 2.5
Global	4.5	93	−88.5	27.5	37.5	−10
Constructive	0	0	0	10	33	−23
Errors:						
Line dissection	51.5	5	+46.5	40	19	+21
Form dissection	11	0	+11	2	0	+ 2
Jumps	48.5	0	+48.5	37.5	25	+12.5
Corrections	42	2	+40	52	37.5	+14.5

* The data for the children were obtained from the published report of Werner and Strauss (8).

† Cf. Footnote 3.

gators, no comparison with regard to this variable is possible. The data involved in these comparisons are presented in Table 7.[3] Unfortunately, the statistical presentation of Werner and Strauss is incomplete and variance is unreported. This makes it impossible to determine the significance of differences between the children's groups. Such a lack also makes it impossible to compare the differences in childrens' groups with those obtained by us with adults. Therefore, the data presented in Table 7 are merely suggestive and accessible only to qualitative consideration.

It appears from an examination of Table 7 that there are differences between the performances of the group of brain-damaged children and their controls. For example, the control group of children had line dissection "errors" on five per cent of their test designs, while the brain-damaged group of children gave line dissections on over half of their designs. In all categories the experimental group showed a considerably greater amount of inadequate behavior. A comparison of the behavior of the two adult groups shows the same trend.

A further inspection of the findings in Table 7 indicates that, while there are differences between the performance of experimental and control groups for both children and adults, the amount of divergence in behavior appears to be considerably greater for the children. Except for form dissection, the difference percentages for the error categories for the children suggest more deviant behavior on the part of the early brain-damage group. It can be suggested, then, that the early brain-damage group shows a greater degree of deficit in terms of errors on the marble board task than does the late brain-damage group.

The greater deficit in the early brain-damage group is most pronounced in the comparison of the method used in the construction of the test designs. As far as method of construction of the test designs is concerned, the early brain-damage group used an incoherent method considerably more frequently than did the control groups of children. Conversely, the early brain-damage group used the global method considerably less frequently than did the control group of children.

The method of construction of the test designs used by the adult groups shows a dis-

[3] It was necessary to reorganize Werner and Strauss' table on the number of figures that showed incoherence in the light of the explanatory text. Werner and Strauss combined "line dissection" and "incoherence" as differentiating responses of the early brain damage group and contrasted this behavior with the 93 per cent global performance in the control group (8). The category of "incoherent" in Table 7 consequently combines those figures scored as "incoherent" and those scored as "line dissection." Although the explanation in the paper by Werner and Strauss is not sufficiently comprehensive, it is apparent that this represents the principle of their findings as revealed in a subsequent paper (9) in which the investigators remark that the brain-damaged group of children "showed *some form* of incoherent procedure on 70 per cent of the figures" (9, p. 213).

similar pattern. It should be noted that incoherent behavior was present in the adult control group to some extent. The difference percentages show significant deviations in the two end steps, *i.e.*, the late brain-damage group gave considerably more incoherent responses and considerably fewer constructive responses.

On the basis of this analysis, it can be suggested that the outstanding differentiating method factor between the early brain-damage group and the late brain-damage group is in the tendency to use the incoherent method.

It is also of importance to note that while the late brain-damage group used an adequate method (constructive or global) in approximately one-third of the designs, almost none of the brain-damaged children are reported to have done so. This seems to be an important difference in the effect of early as contrasted with late brain damage. Clinical descriptions of some varieties of brain-injured children often stress hyperactivity and distractibility as characteristic modes of behavior (1, 3, 5). Such qualitative features of behavior were not explicitly measured by Werner and his associates. It may be, however, that the incoherent approach used by many brain-damaged children in their construction of the marble board designs reflects the effects of their hyperactivity and distractibility rather than perceptual or reconstructive difficulties. The brain-damaged adults in the present study were not characterized by hyperactivity or distractibility so that these behavior manifestations are not variables, as they might have been with Werner's children. It is consequently possible that the differences found between the early brain-damage group and the late brain-damage group in the approach to the marble board task or not due to differences in perceptual difficulties but rather to differences in distractibility and hyperactivity. Such a hypothesis, however, would lie at the foundation of a new experiment.

SUMMARY AND CONCLUSIONS

In a study of marble board behavior of a group of 20 individuals who sustained cerebral damage in later maturity and a group of children with early damage, it was found, using a method we have called *pseudo-comparison* (comparison of each experimental group, children and adults, with their respective controls and using the difference between each brain-damaged group and its control), that brain-damaged children showed a greater deficit in the visual reconstruction task (marble board) than did the adult brain-damaged group. When the adult brain-damaged group was compared with its control group, it was found that as a *group* the experimental subjects showed significantly greater deficit than did the controls. The general hypothesis supported by this study is that the time in life at which a neurologic insult is sustained has a significant bearing upon the behavioral consequences of the damage.

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THE APPLICATION OF THE HEWSON RATIOS TO THE WAIS AS AN AID IN THE DIFFERENTIAL DIAGNOSIS OF CEREBRAL PATHOLOGY

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In 1949 Hewson (6) published a technique which she had developed for the diagnostic determination of cerebral pathology. This procedure is based on the weighted scores of the Wechsler-Bellevue Scale obtained by the patient, the scores being used in various combinations to yield eight ratios, which are then combined in a summary diagnostic judgment of the patterns found in these ratios. This summary diagnostic judgment is termed the J-score, and the diagnosis of cerebral pathology, normal functioning or functional disorder is based upon it according to specific rules made by Hewson. In spite of the three possible diagnostic categories for the J-score the chief role of these ratios is an attempt to discover brain damage rather than to differentiate the psychogenic disorders from normal functioning. For practical purposes, therefore, the J-score may be regarded either as "organic" or as "non-organic." Hewson's basic assumption seems to be that brain-damaged patients demonstrate a specific cognitive deficit in certain areas of functioning, such as motor speed and "new" learning, and if there is an all-over, general intellectual deterioration then these specific cognitive deficits are proportionally greater in comparison to the patient's level of functioning in other areas, such as past memory. However, Hewson also appears to believe that cerebral pathology can exist without necessarily causing any overt intellectual deficit on the usual intelligence tests. Many investigators hold similar views, and Weinstein and Teuber (10) have supported this position experimentally, although they do consider that actually there may be an absolute cognitive impairment, which is not measured by the standard intelligence scale.

Four validation studies of the Hewson Ratios have appeared, two of which have been favorable and the other two disapproving. Gutman (4) and Nicholas and Thetford (9) found that Hewson's J-score agreed with the clinical diagnosis in 71 per cent and 68 per cent of their cases respectively, both results being statistically significant at the .02 level. Gutman, using 30 organic and 30 control cases, compared the differential diagnosis of organic brain damage by three methods, namely those of Wechsler, Reynell and Hewson, all of which are based on the Wechsler-Bellevue weighted scores, and concluded that the only technique feasible for clinical use is that of Hewson. Nicholas and Thetford, however, who examined 106 patients of whom 21 were organic, point out that 32 per cent of their controls were wrongly classified by the J-score as brain-damaged. Both studies emphasize, that, although the Hewson Ratios could differentiate significantly between groups of organics and non-organics, their value is limited in the diagnosis of the individual patient, unless they are used together with other psychological tests or clinical procedures.

The article by Wheeler and Wilkins (11), on the other hand, is considerably more critical of the Hewson Ratios than the two previous studies. Wheeler and Wilkins used 236 non-organic subjects and they found that the Hewson Ratios yielded an organic pattern in 44 per cent of functional psychot-

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ics, 26 per cent of neurotics and 21 per cent of normals. They considered that, although the Hewson Ratios were rather a sophisticated diagnostic method, they misclassified large numbers of non-organic subjects as organic, and that the ratios were almost incapable of diagnosing any individual case by itself.

Recently McKeever and Gerstein (7) carried out a validity study of the Hewson Ratios in which they paid particular attention to the effects of age and intelligence on the J-score. They tested 50 schizophrenic and 52 organic patients, matched for age and intelligence, and found 1) that the over-all accuracy of the Hewson Ratios' diagnoses was 55 per cent, or essentially chance; 2) that among the schizophrenic group one-third of those with an I.Q. of 100 or above and three-quarters of those with an I.Q. of 99 or below were misclassified as organic by the J-score; and 3) that there was a point biserial correlation of .45 and −.34 respectively between organic performance on the ratios and age and I.Q., both significant beyond the .01 level. McKeever and Gerstein therefore concluded that organic performance on the Hewson Ratios varies with age and intelligence and that this method does not differentiate clinical groups beyond a chance level.

It is considered that none of the foregoing evaluative studies was sufficiently exhaustive to yield a thorough appraisal of the Hewson Ratios. Hewson's (6) original work as well as the subsequent validity studies, except that of McKeever and Gerstein (7), can be criticized 1) for not investigating the possible contamination of the data by extraneous factors, such as sex, age and intelligence; 2) for lacking statistical rigor; and 3) for using inappropriate control groups. It is known, for instance, that age factors do influence the weighted scores of the Wechsler-Bellevue Scale (3), and it can easily be shown by means of standard scores (2) that for a given standard score the equivalent Wechsler-Bellevue subtest weighted score may change at different age levels. In other words, on a given subtest both a young and an old individual may perform at similar levels relative to other people of their respective ages, and yet obtain different weighted scores.[2] The Digit Symbol subtest of the Wechsler-Bellevue is especially liable to produce changes in its weighted score with changes in age, and as this subtest is used in the determination of seven out of the eight Hewson Ratios it can be predicted that an older person is more likely to obtain an organic J-score than a younger one, irrespective of his clinical diagnosis.

In this study the Hewson Ratios were applied experimentally to the WAIS, which is the new form of the Wechsler-Bellevue Scale and which has appeared since Hewson's original work. The WAIS contains both scaled scores, which are comparable to the weighted scores of the older Wechsler-Bellevue, and age-scaled scores, which are based on statistical units of dispersion and correct each subtest scaled score for age: therefore the use of the latter should minimize the effect of age on the ratios. The WAIS, moreover, is likely to be used more frequently than the older Wechsler-Bellevue; thus if it could be shown that Hewson's Ratios may be applied to the WAIS they could continue to serve a useful purpose. Finally a new summary diagnostic judgment score will be suggested, which should be used in addition to the J-score to increase the diagnostic efficiency of the ratios.

PROCEDURE

SUBJECTS

The main group of subjects consisted of 123 adult patients from the Neurological Institute,[3] of whom 65 were classified as

[2] In calculating the Wechsler-Bellevue I.Q. the age factor is naturally taken into consideration, but the correction for age is only made to the summed total, verbal and performance weighted scores, and not to each subtest weighted score.

[3] Grateful acknowledgement is extended to Dr. H. Houston Merritt, Dean of Columbia Univer-

"organic" and 58 as "non-organic." The classification of each patient was based on the hospital discharge or final diagnosis made by the neurologist. Only those patients were included in the experimental organic group whose diagnosis was either unequivocal (as confirmed by surgery, autopsy, etc.) or supported by at least two independent and objective clinical or laboratory findings (such as the electroencephalogram, pneumoencephalogram, skull x-ray, arteriogram and lumbar puncture). On the other hand only those patients with definitely negative neurological findings were included in the main, non-organic control group. Forty-seven additional patients not meeting these strict diagnostic criteria were excluded from this study. The experimental organic group was composed of the following clinical categories, *viz.* focal cerebral lesion (*e.g.* oligodendroglioma, tuberculoma, etc.), seven; generalized cerebral involvement (*e.g.* cerebral arteriosclerosis, cerebral vascular accident, cerebral palsy, etc.), 34; convulsive disorder (*e.g.* grand mal, psychomotor, etc.), 18; cerebral multiple sclerosis, three; and Parkinson's Disease, three. The control non-organic group consisted of: psychoneurosis, 21; psychosis, eight; behavior problem, two; and non-cerebral organic diseases (*e.g.* hypothyroidism, avitaminosis, syringomyelia, etc.), 27. Table 1 gives the sex, age and intelligence distribution for all subjects.

An additional group of 27 chronic arthritic patients from Bellevue Hospital[4] was also included in this study, of whom 20 had rheumatoid arthritis, three osteoarthritis, two ankylosing spondylitis, one gout and one Reiter's syndrome. No cases of systemic lupus erythematosus were included, to avoid complications from possible cerebral involvement. Twenty-two of the arthritic patients had no known history of a cerebral involvement and were also free from motor disabilities of their upper extremities, thus forming an additional non-organic group. Three other arthritic patients also had no known history of cerebral trauma, but their fingers were severely crippled, and they were studied as additional non-organics with a manual motor deficit. Finally, two arthritic patients had suffered cerebrovascular accidents and thus served as additional organics. It must be emphasized that, except for the latter two patients, the arthritic group was not screened neurologically, and therefore the remote possibility that more than two of them may have had some cerebral pathology can not be entirely dismissed. Table 1 also lists the sex, age and intelligence distribution of the arthritic subjects.

METHOD

Each subject was given the WAIS and two sets of the Hewson Ratios were calculated, one set being based on the ordinary scaled scores and the other on the age-corrected scaled scores. Hewson's (5) recent minor modification of her rules[5] was adhered to in the determination of the J-scores for both sets of ratios.

A new summary diagnostic judgment, termed the W-score, was formulated and applied experimentally to the data obtained from the age-corrected WAIS scaled scores. The specific and only function of the W-score is to reduce the number of false positives yielded by the J-score (*i.e.* to reduce the proportion of non-organic subjects who erroneously obtain organic J-scores). The W-score is based on the present sample and has not been cross-validated, and therefore any results involving this score must be interpreted with the greatest caution, and

sity College of Physicians and Surgeons and Professor of Neurology, for his permission to publish these findings.

[4] I am indebted to Dr. Currier McEwen, Professor of Medicine and Chairman, Rheumatic Diseases Study Group, New York University School of Medicine, for his permission to publish the data pertaining to the arthritic patients.

[5] Hewson's modification only affects Ratio 1, which now suggests cerebral pathology when the Digit Symbol subtest score is two points or more below the patient's average of all his weighted scores.

TABLE 1

Age, Sex and Intelligence Distribution For All Subjects

Subjects	Age			Mean WAIS I.Q.		
	Mean (Years)	Median (Years)	Range (Years)	Full Scale	Verbal Scale	Performance Scale
A. Experimental Organics						
Total (N = 65)	41.9	43.0	18–69	87	93	81
Males (N = 48)	44.0	44.5	18–69	89	95	83
Females (N = 17)	36.0	32.0	18–65	80	88	73
B. Control Non-Organics						
Total (N = 58)	34.9	35.5	16–64	103	107	98
Males (N = 25)	44.4	41.5	17–64	106	110	100
Females (N = 33)	30.7	28.5	16–63	101	104	96
C. Additional Control Arthritics						
Total (N = 22)	43.2	43.0	19–65	92	94	90
Males (N = 10)	41.2	38.0	27–58	96	97	94
Females (N = 12)	44.8	46.5	19–65	89	92	87
D. Additional Arthritics with Crippled Fingers						
Total (N = 3)	46.0	47.0	34–57	89	90	90
Males (N = 1)	47.0	—	—	89	86	95
Females (N = 2)	34.0; 57.0	—	—	93; 86	94; 91	93; 82
E. Additional Arthritics with Cerebral Pathology						
Females (N = 2)	41.0; 62.0	—	—	77; 90	86; 90	69; 91

at best can only suggest a possible diagnostic device which requires future validation.

The rules for the determination of the W-score are: 1) The W-score can only be calculated when the J-score is found to be organic, provided that the Hewson Ratios are based on the age-corrected WAIS scaled scores (*i.e.* the W-score can not be used with the Wechsler-Bellevue Scale nor with the ordinary WAIS scaled scores); 2) If Rule 1 is obeyed then the W-score suggests cerebral pathology, (a) when Hewson's Ratios 1 and 2 are omitted; (b) if either or both of Hewson's Ratios 5 and 8 are organic according to Hewson's rules; and (c) if one or more of Hewson's Ratios 3, 4, 6 and 7 are also organic according to Hewson's rules. Therefore the W-score does not require additional arithmetical calculation once the ratios have been determined.

RESULTS

In the analysis of the data the J-scores were termed either "organic" or "non-organic," the latter category comprising both Hewson's "functional" and Hewson's "normal" J-scores. Similarly each subject was designated either "organic" or "non-organic" on the basis of the clinical diagnosis. It follows that no distinction was made among the controls between normality and non-cerebral pathology. It must be emphasized, however, that in the statistical treatment of the data due allowance was made that there are actually three, instead of only two, diagnostic Hewson categories, and this is clearly shown in the following tables.

ORDINARY J-SCORES

In this study 67 per cent (100 cases) of all 150 subjects were correctly classified by Hewson's J-scores, when the latter are based on the ordinary scaled scores of the WAIS,

hereafter referred to as "ordinary" J-scores, and this is significant at the .01 level (chi-square = 16.7). Therefore these results are very similar to those of 71 per cent and 68 per cent respectively reported by Gutman (4) and by Nicholas and Thetford (9), as well as to that of 66 per cent obtained by Wheeler and Wilkins (11), when the latter's findings are modified by sorting their data into organic-non-organic Hewson categories. All of the above investigators, like Hewson (6), used the weighted scores of the Wechsler-Bellevue, and the similarity of their results to those of the present study suggests that the Hewson Ratios may be used with the WAIS as well as with the Wechsler-Bellevue.

Section A of Table 2 shows that 79 per cent of the experimental organic group was correctly identified by the ordinary J-scores, but that the latter also produce a very large proportion of false positives among the control groups, as high as 55 per cent for the arthritic controls. This could suggest that the ordinary J-scores lack fine diagnostic discrimination and that they seem biased in favor of organic rather than non-organic ratios.

Division of the subjects by age into young and old categories (the former ranging in age from 16 to 44 and the latter from 45 to 69 years) showed that 70 per cent and 62 per cent respectively of all 89 young and 61 old individuals were correctly classified by the ordinary J-scores. This eight per cent difference in diagnostic accuracy between the two age groups may not appear to be very important, but when the findings are presented for the various clinical groups separately, as shown in Section A of Table 3, then striking age differences appear. Ninety-one per cent old but only 67 per cent young brain damaged patients were correctly identified by the ordinary J-scores, and even more important is the fact that false positives occurred in 26 per cent and 36 per cent respectively of the young compared to as many as 67 per cent and 73 per cent respectively of the old non-organic controls and arthritics. Similarly, for all 150 subjects the ordinary J-scores misclassified as many as 71 per cent old compared to only 29 per cent young controls, and nine per cent old to 32 per cent young organic patients, significant at the .05 level (chi-square = 8.9). An old non-organic individual is thus about

TABLE 2

Classification of All Subjects by Hewson's J-Score and by the New W-Score

Classification by Hewson's Ratios	Experimental Organics (N = 65) Per cent	Control Non-Organics (N = 58) Per cent	Additional Arthritic Controls (N = 22) Per cent	Extra Arthritics	
				Crippled Fingers (N = 3)	Cerebral Pathology (N = 2)
A. J-Scores based on ordinary WAIS Scaled Scores					
1. Organic J-Score	79	36	55	3*	2*
2. Normal J-Score	06	17	14	0	0
3. Functional J-Score	15	47	32	0	0
B. J-Scores based on age-corrected WAIS Scaled Scores					
1. Organic J-Score	62	17	32	2*	2*
2. Normal J-Score	11	36	18	0	0
3. Functional J-Score	28	47	50	1*	0
C. W-Scores based on age-corrected WAIS Scaled Scores					
1. Organic W-Score	45	02	09	0	2*
2. Non-Organic W-Score	55	98	91	3*	0

* These are the actual numbers of subjects and not percentages.

TABLE 3

The Effect of Age on the Classification of all Subjects by Hewson's J-Score and by the New W-Score

Classification by Hewson's Ratios	Experimental Organics		Control Non-Organics		Additional Arthritic Controls*	
	Young (N = 33) Per cent	Old (N = 32) Per cent	Young (N = 43) Per cent	Old (N = 15) Per cent	Young (N = 11) Per cent	Old (N = 11) Per cent
A. J-Scores based on ordinary WAIS Scaled Scores						
1. Organic J-Score	67	91	26	67	36	73
2. Normal J-Score	12	00	14	27	18	09
3. Functional J-Score	21	09	61	07	46	18
B. J-Scores based on age-corrected WAIS Scaled Scores						
1. Organic J-Score	61	63	21	07	27	36
2. Normal J-Score	06	16	23	73	18	18
3. Functional J-Score	33	22	56	20	55	46
C. W-Scores based on age-corrected WAIS Scaled Scores						
1. Organic W-Score	39	50	02	00	09	09
2. Non-Organic W-Score	61	50	98	100	91	91

* The 5 extra arthritic patients with crippled fingers or cerebral pathology are not included. Age Range: Young: 16 to 44 Years; Old: 45 to 69 Years.

two and one-half times as likely to obtain a false positive ordinary J-score than a young non-organic subject. These results therefore, suggest strongly that age is directly related to organic performance on the Hewson Ratios, when the latter are based on the ordinary scaled scores of the WAIS. McKeever's and Gerstein's (7) findings, indicating a point biserial correlation of .45 between age and organic performance on the ratios, are thus strongly supported.

Table 4 was compiled to investigate possible influence due to intelligence. All subjects were divided on the basis of the WAIS Full Scale I.Q. into two categories: high (I.Q. = 100 or above) and low (I.Q. = 99 and below). Section A of Table 4 shows that the percentage incidence of ordinary J-scores is fairly similar for the high and low intelligence categories of the various clinical groups. The effect of intelligence on the ordinary J-scores for all subjects was found to be statistically insignificant (chi-square = 0.2). Therefore, in this case, McKeever's and Gerstein's (7) result, that intelligence is inversely related to organic performance on the ratios, was not supported.

The possibility of a sex factor's influence on the present results was found to be negligible: the over-all accuracy of the ordinary J-scores for all 150 cases was 68 per cent for men and 65 per cent for women, and this small difference did not reach statistical significance.

AGE CORRECTED J-SCORES

When Hewson's Ratios are based on the age-corrected scaled scores of the WAIS, then the J-scores, hereafter referred to as the "age-corrected" J-scores, correctly classify 71 per cent (106 cases) of all 150 subjects. This represents a very small increase in the accuracy of the Hewson categories over the ordinary J-scores. Inspection of Section B of Table 2 demonstrates that the age-corrected J-scores are superficially less sensitive to cerebral pathology than the ordinary J-scores, but on the other hand they reduce by half the number of false positives among the control groups, and this gives them a

TABLE 4

The Effect of Intelligence on the Classification of all Subjects by Hewson's J-Score and by the New W-Score

Classification by Hewson's Ratios	Experimental Organics		Control Non-Organics		Additional Arthritic Controls*	
	High (N = 17) Per cent	Low (N = 48) Per cent	High (N = 33) Per cent	Low (N = 25) Per cent	High (N = 7) Per cent	Low (N = 15) Per cent
A. J-Scores based on ordinary WAIS Scaled Scores						
1. Organic J-Score	77	79	39	32	43	60
2. Normal J-Score	06	06	15	20	29	07
3. Functional J-Score	18	15	46	48	29	33
B. J-Scores based on age-corrected WAIS Scaled Scores						
1. Organic J-Score	59	65	15	20	29	33
2. Normal J-Score	12	08	49	20	29	13
3. Functional J-Score	29	27	36	60	43	53
C. W-Scores based on age-corrected WAIS Scaled Scores						
1. Organic W-Score	24	54	00	04	00	13
2. Non-Organic W-Score	76	46	100	96	100	87

* The 5 extra arthritic patients with crippled fingers or cerebral pathology are not included. Intelligence: High: WAIS Total I.Q. = 100 or above; Low: WAIS Total I.Q. = 99 or below.

small diagnostic advantage over the ordinary J-scores.

The age-corrected J-scores are, by definition, already based on scaled scores adjusted for age, and an additional check demonstrates that the effect of age is negligible, because 71 and 70 per cent respectively of all young and old subjects are correctly identified. Furthermore, 62 and 64 per cent respectively of the young and old organic patients, and 76 and 79 per cent respectively of all young and old controls, are now correctly classified. Section B of Table 3 gives a further breakdown of the results by clinical groups, but the percentages for the two control groups may be somewhat misleading, as they suggest that three times the number of young compared to old non-organics, and only three-quarters of young compared to old arthritics, yield false positives. However, these two mutually opposing trends of the two control groups cancel each other out when all controls, including the three crippled arthritic patients, are considered together, furnishing the above stated results. Furthermore, the old non-organic control subgroup from the Neurological Institute contained only 15 patients, compared to 43 patients in its young component subgroup, and thus if there had been even only one more false positive among the old subgroup then the percentage misclassification would have been doubled from seven to 14. Similarly, there were only 11 subjects in each of the two age subgroups for the arthritic controls, and therefore too much weight cannot be attached to apparent large percentage differences among the control subgroups, because a very few individuals would yield large percentage differences of the small subgroup totals involved. Therefore the important finding is that based on all the control cases.

Intelligence, as measured by the WAIS Full Scale I.Q., was found not to be a contaminating variable: because 75 and 69 per cent respectively of all high and low intelligence cases were observed to be accurately classified by the age-corrected J-scores, and this is not significant (chi-square = 0.4).

However, Section B of Table 4 suggests that for each clinical group those subjects in the high intelligence category obtained organic age-corrected J-scores less frequently than those in the low intelligence category, but this also fails to reach statistical significance (chi-square = 1.1). Therefore, McKeever's and Gerstein's (7) finding, that organic performance on the ratios varies with intelligence, is not really supported here, although such a trend may actually exist.

The sex factor was found to be negligible for the age-corrected J-scores, as 71 per cent of men and 70 per cent of women were accurately identified.

W-SCORES

This new summary diagnostic judgment was accurate in 111 (74 per cent) of all cases. However, it may not be meaningful to consider the accuracy of the W-scores as a percentage of all 150 subjects, because these scores can, by definition, only be calculated when the age-corrected J-score is organic. (All non-organic age-corrected J-scores would invariably yield non-organic W-scores, thus making the calculation of the latter superfluous.) Therefore, as 61 organic age-corrected J-scores were obtained in this study, only an equal number of W-scores was determined. Forty-seven (77 per cent) of these 61 W-scores were diagnostically correct, which is statistically significant (chi-square = 17.9; $p = .001$), and compares favorably with the 69 per cent (42 out of 61) accuracy of the organic age-corrected J-scores. There were altogether 34 organic and 27 non-organic W-scores, and of those only three were false positives, compared to 34 per cent (21 cases) of false positives among the organic age-corrected J-scores. (These figures must not be confused with those given in the previous section in which the accuracy of all, and not only organic, age-corrected J-scores was discussed.) The present results thus suggest tentatively that nine per cent of organic W-scores are false positives, but that for all control subjects only four per cent (3 out of 83) false positive W-scores occur.

Section C of Table 2 shows that there were some minor differences among the two control groups, as only one of the 58 non-organic patients from the Neurological Institute, and two of the 22 arthritic controls from Bellevue Hospital yielded false positive W-scores. Similarly it is of some interest that the W-scores distinguished correctly between the two arthritic patients with cerebral vascular accidents and the three arthritic patients with crippled fingers, whereas the age-corrected J-scores misclassified two of the latter (Table 2). Again, however, the number of cases is too small to make this a significant difference.

Age did not appear to affect the W-scores significantly, because they correctly classified 75 per cent of the young and 72 per cent of the old subjects. Furthermore, there was also no significant difference in the proportion of the organic W-scores between the young and old individuals (chi-square = 1.9). The effect of intelligence on the organic W-scores was also found to be statistically insignificant (chi-square = 4.7; df = 5; $p > .30$). Section C of Table 4, however, suggests that there is a slight trend for individuals in the low intelligence subgroups to obtain proportionally more organic W- scores than individuals in the high intelligence subgroups. This is probably associated with the similar trend for the age-corrected J-scores. Sex did not significantly influence the W-scores.

CLINICAL DIAGNOSIS

Investigating the possibility of differential functioning on the Hewson Ratios among the various clinical subgroups, it was found that the age-corrected J-scores correctly identified respectively 71 per cent (5 in 7) and 72 per cent (26 in 36) of the patients with focal cerebral lesions and with generalized cerebral pathology (including the two arthritic patients with cerebral vascular accidents). Similarly the organic

age-corrected J-scores for these two brain damaged subgroups yielded respectively 80 and 85 per cent positive W-scores; or, on the basis of the total number of subjects in each of these two clinical sections, the accuracy of the W-scores was respectively 57 and 61 per cent. (It must be reiterated that the W-scores are based on the organic age-corrected J-scores, and thus the former can never exceed the latter in number.) However, only ten of 18 patients with convulsive disorders had positive age-corrected J-scores, the latter yielding 50 per cent organic W-scores, and for all cases in this section the W-scores were correct only to the extent of 28 per cent. Similarly, only two of the six patients with cerebral multiple sclerosis or Parkinson's Disease had organic age-corrected J-scores, and of those only one subsubject had a positive W-score. Comparing the patients with focal or generalized cerebral pathology with those patients suffering from a convulsive disorder, Parkinsonism or multiple sclerosis, the difference in their respective organic age-corrected J-scores fails to reach statistical significance (chi-square = 1.2; $p = .30$). On the other hand, however, the difference in the positive W-scores for all subjects in the above subgroups is significant at the .05 level (chi-square = 4.1), but if only the organic age-corrected J-scores are used as base for the W-scores then this difference in positive W-scores becomes statistically insignificant (chi-square = 1.3; $p = .30$).

It is unfortunate that the number of patients with cerebral multiple sclerosis, Parkinson's Disease or convulsive disorder is too small to allow inferences of clear-cut differences in organic performance on the ratios for patients with various kinds of cerebral pathology. Clinically such inferences seem to be permissible, as supported by the above findings, but statistically they fail to be substantiated, except for the difference in positive W-scores based on all organic patients.

The Hewson Ratios, when applied to all 83 control patients without cerebral pathology, yielded 21 cases (25 per cent) of false positive age-corrected J-scores. There were no statistically significant intersubgroup differences, as five of 21 neurotics, two of eight psychotics, neither of the two patients with behavior problems, five of 27 patients with non-cerebral physical diseases, and nine of 25 arthritics were incorrectly classified as organic by the age-corrected J-scores (chi-square = 3.1). One neurotic and two arthritic patients out of all 83 controls had false positive W-scores. The arthritic controls yielded the largest error in Hewson classification among all control subjects, but perhaps slightly less emphasis should be placed on this result than on those for the other groups, because the arthritic controls were the only subjects in this investigation who were not thoroughly screened neurologically.

This study included eight functional psychotics (six schizophrenics and two depressives), of whom only two (one schizophrenic and one depressive) yielded organic Hewson performances. This finding thus differs from those of McKeever and Gerstein (7) and of Wheeler and Wilkins (11). The former found that one-third of their more intelligent and three-fourths of their less intelligent schizophrenic controls obtained organic Hewson diagnoses, and the latter observed that 44 per cent of their functional psychotics were misclassified as organic by the ratios. The number of functional psychotics in this study is, of course, too small to permit an adequate basis for discussion and of comparison with the previous investigations. It is possible that some of the other investigators' schizophrenic and functional psychotic controls may have had some kind of a pathological cerebral condition as well, which was not discovered due to a lack of adequate neurological screening procedures. McKeever and Gerstein (7), for instance, defined their diagnostic criterion as follows: "adequacy of diagnosis was partially assured by the requirement that each patient have at least

two congruent, independent diagnoses, and never have been diagnosed differently," but they do not state on what basis the diagnoses were made. It would appear, therefore, that their schizophrenic controls were not neurologically checked, and thus their screening procedure may have lacked thoroughness, as their diagnostic criterion apparently did not require confirmation by objective tests.

DISCUSSION

There is a lack of adequate psychological tests of early brain injury. It is doubtful if a psychological test will ever be designed to yield anything close to a perfect diagnostic classification of cerebral pathology, especially as many studies, *e.g.* that by Weinstein and Teuber (10), have shown that trauma in different brain loci may produce none or different degrees of cognitive (and other mental) deficits, and that trauma in a given brain locus may produce different symptoms in different individuals. Nevertheless, any psychometric device, such as the Hewson Ratios, that does appear to be sensitive to some or many pathological cerebral processes is entitled to a place in the repertory of the clinical psychologist. The Hewson Ratios have the practical advantage of being derived from an intelligence scale, generally given routinely, and the ratios are thus a timesaver for the busy clinician, because only a few extra minutes are required for their calculation. It is quite obvious, however, that the Hewson Ratios should never be used as the sole diagnostic technique in the individual case of suspected cerebral pathology, and that other tests should be administered as well. On the latter point there appears to be complete agreement among most psychologists, including Baker (1), Gutman (4), Hewson (6), and Nicholas and Thetford (9).

Meehl and Rosen (8) in a penetrating discussion indicate many difficulties in the evaluation of the efficiency of the majority of psychometric devices, and the lack of adequate base rates is one of their chief criticisms. Meehl and Rosen define base rates as diagnostic and prognostic statements made on the basis of actuarial or experience tables. They point out that to be efficient any psychometric device must make it possible to give a greater number of correct decisions than could be made solely in terms of base rates. Strictly speaking, the base rates for the Hewson Ratios are not known, and thus it is difficult to evaluate their efficiency, especially that of the age-corrected J-scores and of the W-scores (which in any case still require cross-validation). Fortunately, however, if this discussion is restricted to the application of the Hewson Ratios in cases of suspected cerebral pathology, rather than to a representative sample of the total population or to psychiatric patients, then crude base rates appear to be available. In this study nearly all experimental and control subjects (excepting the arthritic group) had demonstrated some true or false neurological symptoms, resulting in their referral for neurological consultation. Furthermore, in many of these patients the preliminary neurological examination was diagnostically equivocal; consequently laboratory procedures were required for a definite diagnosis. It is probably justifiable to assume that most of these patients were tentatively diagnosed as showing cerebral pathology on the basis of the initial neurological examination until laboratory results were available to support or negate the preliminary diagnosis. Assuming again that about half of these patients were later shown to have cerebral pathology, then the Hewson Ratios, in order to be efficient, would have to classify these patients significantly better than chance and this study suggests that the age-corrected J-scores and the W-scores actually do this. The age-corrected J-scores produced 34 per cent false positives and 37 per cent false negatives while the W-scores yielded four per cent false positives and 54 per cent false negatives, compared to a crude base rate of about 50 per cent false positives. These results suggest a certain amount of

diagnostic efficiency. The assumption that half of the doubtful cases were organic is, of course, not established but inferred—somewhat tenuously perhaps—from the proportion of organic to nonorganic patients (excepting the arthritic group).

In conclusion, the results of the present study demonstrate that the Hewson Ratios may be applied to the WAIS, and that the age-corrected J-scores show a higher validity than the ordinary J-scores. Pending future cross-validation, the new W-score suggests a method for reduction of false positives, as this score emphasizes a given individual's proportional poor functioning in two or more cognitive areas, such as speed and new learning, compared to his functioning in certain other areas, such as past memory, whereas Hewson's original J-score only stresses one sphere of decreased cognitive functioning in order to be positive. Furthermore, the results of this study strongly suggest the need for further refinement of existing psychological techniques for the detection of early brain damage, and for considerably more careful evaluation of possible differential psychological functioning of patients with various types of cerebral pathology. In general, much more care in the screening of controls seems to be required to ensure a lack of contamination by unsuspected brain pathology. The deteriorative effects of the "normal" aging processes on differential cognitive functioning also requires further investigation in order to allow better estimation of deviations from the normal range in the older patients. Finally, the effect of intelligence should also be investigated more thoroughly, especially as superior intelligence may mask the decline in some or many abilities.

SUMMARY

The Hewson Ratios for the diagnosis of cerebral pathology were applied to both the ordinary and the age-corrected scaled scores of the WAIS, which had been administered to 150 patients, consisting of 65 neurological and 2 arthritic patients with cerebral pathology, and 58 neurological and 25 arthritic patients without cerebral pathology. The Hewson summary diagnostic judgment, when based on the ordinary WAIS scaled scores, was found to be correct in 67 per cent of all cases, and thus similar to results obtained in previous studies by other psychologists in which the weighted scores of the Wechsler-Bellevue scale were used. These ordinary ratios were apparently sensitive to cerebral pathology, but they misclassified a high proportion of non-organic patients, and they were also seriously contaminated by age. However, when the ratios were based on the age-corrected scaled scores of the WAIS, then the Hewson summary diagnostic judgment was shown to have an all-over diagnostic accuracy of 71 per cent. It was not significantly affected by age or sex, but it may have been slightly (although statistically insignificantly) affected by intelligence. A new, modified summary diagnostic judgment was suggested, which reduced the number of false positives (*i.e.*, organic Hewson performance by non-organics) to four per cent, but otherwise lacked sensitivity. This new score, however, still requires cross-validation. It was shown that among patients with cerebral pathology different diagnostic groups may show different patterns of cognitive functioning. About half of the patients with epilepsy, cerebral multiple sclerosis and Parkinsonism gave organic Hewson performances, whereas patients with other kinds of focal or generalized cerebral pathology were correctly identified by the ratios in 72 per cent of the cases. No significant intragroup differences in Hewson performance for the controls were found. Only two of eight functional psychotics gave organic ratios. This differs from the results of two other investigations in which schizophrenics had a very high proportion of false positives, but there were not sufficient functional psychotics in this study to allow an adequate comparison. The difficulty of establishing base rates was discussed. It was

concluded that the Hewson Ratios appear to be a valid technique in the assessment of cerebral pathology when used with the age-corrected scaled scores of the WAIS and when applied to patients with some neurological symptoms or suspected brain injury. However, the ratios should never be used as the sole test of cerebral pathology in the individual case, but given in association with other psychological, clinical or laboratory procedures.

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THE EFFECT OF CHLORPROMAZINE ON THE VARIABILITY OF MOTOR TASK PERFORMANCE IN SCHIZOPHRENICS[1]

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This report will present the findings of a study of the effects of chlorpromazine on the variability of motor functioning in a group of schizophrenic patients. That is, rather than investigating the rate at which a motor task is performed, we will focus on the difference in rates between subjects and the differences in rate in a subject from moment-to-moment. Our interest in variability was stimulated by the frequently appearing report that schizophrenics are more variable than control groups (2, 3, 5, 10). Bellak, in a comprehensive review of the literature related to schizophrenia indicates in his summary (3, p. 445): "The most significant result of all was that almost any data observed showed a greater variability (in schizophrenic groups) than in normals."

In most of the studies cited, group heterogeneity *per se* was not the focus of experimental interest. Usually, the statistic under investigation was the mean. The schizophrenic groups, however, often contain individuals whose scores fall outside the range of those obtained by the controls. When the scores of these deviate individuals in the patient group are located both above and below the range found in the normal control group, they will tend to cancel each other and the average results reported for the patient group may not be significantly different from those of the control group. A concrete example of this may be seen in a study of the effects of injected epinephrine on blood pressure by Silverstein and Kline (10). The range of changes in systolic pressure obtained by these investigators for a schizophrenic population was 73.4 mm. Hg, while the range for the control group was 24.4 mm. Hg. The mean values of these changes for the two groups, however, were almost identical (56.4 mm. Hg and 57.2 mm. Hg respectively). On the behavioral level, Hunt (5) indicates that this trend may be found in some studies of sensory and response processes.

One characteristic, then, which appears common to a wide variety of parameters is that interindividual variability is often greater in schizophrenics than in control groups. Less information is available concerning the intraindividual variability of schizophrenics. In the literature one finds more frequent reference to increases than to decreases in intraindividual variability. Wechsler (11) reports that interest scatter (intraindividual variability) is typical of the intellectual performance of many schizophrenic patients and, as such, may even have some diagnostic value. King, in a discussion of psychomotor functions (6), describes a number of studies in which trial-to-trial and day-to-day variability is found to be higher in disturbed groups.

Many factors could have produced these differences. A schizophrenic group may be more heterogeneous only when compared with a control group selected from a relatively homogeneous population such as a hospital staff. A patient with intermittent hallucinations would respond less consistently than subjects not experiencing this symptom. Or, the finding of increased variability in schizophrenic populations may occur because variability itself is one of the

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criteria in the differential diagnosis of this disease. Insofar as diagnosis is dependent on the individual's deviation from the accepted norm, to this extent will the schizophrenic group show increased heterogeneity. On the other hand, greater variability may occur because of some factor intrinsic to the disease process. It seems unlikely, however, that any single factor can explain all of the findings.

From clinical evidence it appears that the action of tranquilizing drugs might profitably be considered in relation to their effect on group heterogeneity. Tranquilizers such as chlorpromazine have been found clinically useful in stuporous catatonic and in agitated overactive patients (9). With improvement motor activity levels increase in the former and decrease in the latter. The effect of a tranquilizer thus seems to be dependent on the initial state of the patient. In a heterogeneous group consisting of both hyperactive and hypoactive patients, treatment with a tranquilizer should decrease the *heterogeneity* of group motor performance, possibly without appreciably affecting the mean. This may be contrasted with non-selective sedatives which would be expected to decrease, or stimulants which would be expected to increase *mean* motor activity levels of heterogeneous groups.

The action of a tranquilizer might similarly affect the variations in performance of one individual from trial to trial. Since psychiatric illness is often characterized by erratic or rigidly stereotyped motor behavior, drugs such as chlorpromazine might affect those whose performance falls in the extremes of intraindividual variability.

In order to experimentally test these ideas we examined subjects on a finger tapping task. This task was used because 1) it is simple and easily quantified; 2) the distribution of scores could be expected to be nearly symmetrical; 3) it could be frequently repeated with predictable practice effects; 4) it was readily accepted by schizophrenic patients and 5) motor functions seem sensitive to chlorpromazine effects. For the purposes of this study, our main interest was in a comparison between a schizophrenic group treated with chlorpromazine and one treated with placebo. We will also present data on a nonpsychiatric group.

METHOD

Consecutive male admissions to the semi-disturbed ward of Bellevue Psychiatric Hospital were accepted as subjects if they met the following criteria.

1. Unequivocal diagnosis of schizophrenia made independently by two psychiatrists.
2. Age between twenty and fifty years.
3. No history of excessive use of alcohol.
4. No evidence of illness other than schizophrenia.
5. No use of drugs up to one week prior to admission.

Fifteen normal volunteers were selected from the kitchen staff at the hospital, being accepted on the basis of meeting the age criteria and having no overt evidence of illness. Thirty subjects who met the above criteria for inclusion in the schizophrenic group were divided into two sub-groups by alternation of patients. Table 1 presents the composition of the three groups, which did

TABLE 1
Classification of Subjects

Group	N	Age		No. of Right-handed Subjects	Onset of Illness		
		Mean	Range		Acute	Insidious	Indeterminable
Normal controls	15	34.1	24–43	13	—	—	—
Placebo Group 5 Paranoid 0 Schizo-affective 10 Undifferentiated	15	31.9	20–50	14	3	9	3
Chlorpromazine Group 6 Paranoid 1 Schizo-affective 8 Undifferentiated	15	35.5	21–41	12	2	9	4

not differ significantly from each other on any of the criteria considered.

Each group received a pretest (to be described), and was then retested on the fifth day. The patient groups received four days of treatment between pretest and posttest. One group received chlorpromazine at doses of 50 mg. q.i.d. The other group was treated with inactive placebo identical in appearance with chlorpromazine. A double blind method was used throughout.

The tapping board was a 12″ x 36″ box, the top surface of which was formica. Two Veeder Root counters were mounted at one edge of the board, 26″ apart, and were actuated by 1″ plungers. Each subject was tested individually with the tapping board placed before him so that he could comfortably reach the plungers. The plungers were depressed by the index fingers, and each depression was recorded on the counter. Each subject was told, "I want to see how you tap." No effort was made to motivate the subject to tap at maximum speed. When subjects asked questions that were not directly pertinent to the instructions they were given nondirective answers. Subjects tapped for ten-second periods alternately with right hand, left hand and both hands. A counterbalanced order to control for systematic fatigue and practice effects was used (Table 2). Between each ten-second tapping period, there was a ten-second rest period. A trial consists of the sum of each hand separately and both hands together. There were, thus, five trials for each subject in addition to the practice trial. The same procedure was repeated on posttest.

TABLE 2

Rotation Order of Tapping

Trial*	Left-hand	Right-hand	Bimanual
Practice	3	1	2
I	2	3	1
II	1	2	3
III	3	2	1
IV	1	3	2
V	2	1	3

* The roman numbers denote replications. The arabic numbers indicate the order in which the hands were tested. Under bimanual conditions, both scores were obtained at the same time. The data presented is the sum of scores within each trial (1 + 2 + 3).

TABLE 3

Tapping Rate

Group	Pre		Post		Change	
	M	S.D.	M	S.D.	M	S.D.
Control	183.4	39.34	187.0	39.47	3.6	25.64
Placebo	148.4	50.10	156.1	61.17	7.7	21.68
Chlorpromazine	146.6	41.92	159.8	33.32	13.2	34.72

RESULTS

Table 3 presents a summary of the tapping rates of the drug, placebo and control group. Each score represents the average tapping rate of the five trials. It can be seen that on pretest the means of the two schizophrenic groups are similar. However, each patient group is significantly slower than the control group ($p < .05$ for both; t-test). All groups were slightly faster on the posttest. The means of the change of scores were not significantly different from zero for any of the groups. Also, there was no significant difference between the means of the posttest scores of any of the groups. On the basis of group mean scores, chlorpromazine treatment had no significant effect on the motor performance on the tapping test. That is, there was no significant change in tapping rate in the chlorpromazine-treated group, nor did the chlorpromazine-treated patients differ significantly from the placebo-treated patients.

The standard deviation of the chlorpromazine group decreased (Table 3), while that of the placebo group increased on posttest scores. These changes were not significant in themselves but cumulated to produce a highly significant difference ($p < .001$; F test) between the variance of the two schizophrenic groups after treatment although there was not a significant differ-

ence between these groups prior to treatment. That is, while we anticipated a relative decrease in the heterogeneity of the chloropromazine-treated group, the significance of this difference is contributed as much by the increase in the placebo group as by the decrease in the drug group.

Within the chlorpromazine group, those subjects who obtained relatively high pretest scores tended to show the greatest decrement in post treatment scores. Conversely, those with low pretest scores tended to increase on posttest. Product moment correlations were computed using pretest tapping rate as one variable and the difference between pretest and posttest rates as the other. The correlations are presented in Table 4. The negative correlation ($r = -.64$) for the chlorpromazine group indicates that the changes in this group are significantly related to the initial rate. In the placebo group, on the other hand, there is a slight but non-significant positive correlation ($r = +.35$). The correlation for the chlorpromazine group is significantly different from that for the placebo group ($p < .02$, t-test, r to z transformation).

Correlations of prescores with change scores must be interpreted with caution. Lacey (7) has presented a systematic discussion of factors which will tend to produce spuriously high correlations with this type of data analysis. In presenting these correlations we are concerned with the difference between the correlation obtained for the placebo group and that obtained for the drug group rather than the magnitude of either correlation. Since it may be assumed that the effects of the "law of initial values" (7)—that is, spontaneous tendencies for statistical regression—would be the same for the two groups, the difference between the groups may be attributed to differences between the treatments.

In order to investigate the effects of chlorpromazine on intraindividual variability, a variability index (V.I) was computed for each individual. The tapping rate for each subject is the mean of five trials (see Table 2). The V.I. was obtained by dividing the standard deviation of these five trials by the mean tapping rate. This score reflects the tendency of an individual to vary from moment-to-moment.

A product moment correlation between tapping rate and V.I. was computed for all scores of all subjects. This correlation was found to be −.19, indicating that the V.I. is not significantly related to tapping speed. In Table 4 this correlation is reported for each group and it can be seen that the relationship is not significant for any of the groups.

Table 5 presents a summary of the mean V.I. for each of the groups. It can be seen that on pretest the mean V.I. of the two schizophrenic groups is higher than that of the normal group. However, this difference is significant ($p < .05$) only for the chlorpromazine group. All groups obtained lower

TABLE 4
*Product-Moment Correlations**

	Control	Placebo	Drug
Tapping Rate			
Pre - Post	.79	.94	.60
† Pre - Change	−.32	.35	−.64
Variability Index			
Pre - Post	.73	.49	−.46
† Pre - Change	−.85	−.43	−.95
Tapping Rate — Variability Index	−.03	−.25	−.15

* N = 15: an r of .55 is required to reject the null hypothesis at .05 level of confidence.

† A positive correlation indicates that the higher initial scores are associated with the greater increases on posttest.

TABLE 5
Variability Index

Group	Pre		Post		Change	
	M	S.D.	M	S.D.	M	S.D.
Control	0.055	0.0431	0.041	0.0235	−0.014	0.0306
Placebo	0.082	0.0426	0.043	0.0472	−0.039	0.0457
Chlorpromazine	0.112	0.0716	0.056	0.0334	−0.056	0.0920

mean V.I. scores on posttest. This drop in V.I. was not significant for any of the groups. Also, there was no significant difference between mean V.I. of any of the three groups on posttest. In other words, chlorpromazine treatment was not shown to differentially affect *average* V.I. values. However, the distribution of V.I. scores of the chlorpromazine group became more homogeneous on posttest while the placebo group was unchanged. This is demonstrated by a significant change in the standard deviation of the V.I. for the chlorpromazine group (t-test for correlated variances, $p < .001$), while the standard deviation of the placebo group did not change. The standard deviation of the V.I. for the normal group also dropped significantly ($p < .01$) on posttest. None of the posttest standard deviations for any of the groups differs significantly from each other.

We also anticipated that the changes produced by chlorpromazine treatment would be related to the initial V.I. In order to demonstrate this, we correlated the pretest V.I. with the change in V.I. from pretest to posttest. In Table 4 these correlations are presented. A significant correlation ($r = -.95$) was obtained between the prescore and change score of the chlorpromazine group. In the placebo group, on the other hand, the correlation ($r = -.43$) between pre and change V.I. scores is not significant. The difference between the correlation of the placebo group and that of the chlorpromazine group is significant at the .01 level of confidence (t-test, r to z transformation). A significant correlation ($r = -.85$) exists between prescore and change score of the V.I. for the normal group.

DISCUSSION

Behavioral changes accompanying drug treatment are complex. In order to interpret the data we have presented, it is important to remember that the most characteristic effect of practice on a motor task is an increase in the rate and a decrease in the trial-to-trial variability. All groups showed such tendencies, although the changes in the group means were in no case significant. While practice effects would tend to mask treatment effects, the chlorpromazine group appears to differ from the placebo group in characteristic ways on posttest. From the "law of initial values" (7), one would expect that the individual with the lower prescore would tend to have the bigger change score; thus producing a negative correlation between prescore and change score and a regression toward the mean. For both tapping rate and V.I., the chlorpromazine-treated group showed a greater tendency for regression toward the mean than did the placebo-treated group. Both tapping rate and V.I. mean scores on posttest were not significantly different from the mean pretest scores. A decrease in variance without change in the mean will result in individual scores clustered closer to the mean. This tendency was shown in the chlorpromazine-treated group for both the tapping rate and the V.I. For the former it was significant only in relation to the placebo-treated group (which showed a rise in variance), and for the latter only in relation to pretest score. Therefore, while it appears that within the limitations of the subjects and procedures of this study, a chlorpromazine effect can be distinguished from the practice effect and the influence of "initial values," the effect is not a simple one.

The findings in this study with a finger tapping test are considerably reinforced by the findings of Heilzer (4), published after this study was undertaken. This investigator, using jump reaction time, found a decrease in group variability after treatment with chlorpromazine. The placebo group did not change significantly. He reports that intraindividual variability for the drug-treated group as a whole, decreased but the least variable subjects tended to become

somewhat more variable. In many respects, our data parallels those reported by Heilzer.

We have indicated that it is very difficult to determine if the greater variability found among schizophrenics is an artifact of the experimental procedures, an epiphenomenon of the disease or whether it is related to some process intrinsic to the disease. While our data contribute little to a choice between these alternatives, the study of variability holds promise as a sensitive method for the understanding and evaluation of tranquilizing action.

In the data reported here, neither the mean tapping rate nor mean variability index were significantly affected by chlorpromazine. By the usual methods for treating this type of data *(i.e.*, comparison of means), chlorpromazine would be considered devoid of demonstrable activity. It is possible that tranquilizing action, in general, would be better understood if the changes produced by these drugs are considered in relation to the initial state of each subject.

The study of intraindividual variability may be especially useful in the evaluation of biological data. All functions—biological and psychological—fluctuate from time to time in the same subject independently of errors of measurement. Anderson *et al.* (1) have demonstrated this in a study of blood glucose. They found striking periodic oscillations in blood glucose levels which were not reflected adequately by single tests or mean values.

On certain biological measures schizophrenics may fluctuate around the same mean as normals but differ in the range or rhythmicity of these fluctuations. A consideration of both intra- and interindividual variability would provide one approach to the study of these processes.

SUMMARY

The performance of 15 normal subjects, 15 schizophrenic patients treated with placebo, and 15 schizophrenics treated with chlorpromazine was evaluated on a finger tapping test.

The chlorpromazine-treated group of patients decreased in heterogeneity, while the placebo group increased somewhat.

In the chlorpromazine-treated group a significant negative correlation was found between initial tapping rate and change in rate following treatment. Placebo-treated patients obtained a non-significant positive correlation.

A variability index was computed for each subject. The chlorpromazine group obtained more homogeneous scores on this measure on posttest.

The placebo group and the chlorpromazine group differed from each other in regard to the magnitude of the correlation between prescore and change score of the V.I.

It is suggested that consideration of variability is important in the evaluation of the effect of tranquilizers on performance tasks. Variability is also an important consideration in other biological and psychological procedures.

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RELATION BETWEEN THRESHOLD AND DURATION OF SEIZURES AND ELECTROGRAPHIC CHANGE DURING CONVULSIVE THERAPY

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Previous studies have emphasized the individual variability in the electrographic response during a course of convulsive therapy. Some patients develop large amounts of slow wave activity early in treatment, while in others such changes occur late or not at all (4, 6). Factors responsible for this variability are not clearly understood. Our own data and those of others suggest that chronological age and the characteristics of the pretreatment EEG play a role but even after consideration of these factors variability still persists (3, 4, 6, 9, 11, 13).

We have postulated that individual differences in nervous system reactivity may be significant. These differences should thus be reflected in responses other than the degree of electrographic change during electroshock.

Accordingly it was suggested that certain features of the convulsion itself might bear a significant relationship to EEG reactivity during electroshock. Two features were investigated in the present study: 1) the threshold of the initial seizure; 2) the duration of the seizures. The hypothesis was that patients showing a high degree of EEG reactivity, *i.e.*, a high degree of EEG slow wave activity early in the course of treatment, would have significantly lower convulsive thresholds and longer seizures than patients with low degrees of EEG induced slow wave activity.

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MATERIAL AND METHOD

Thirty-nine patients receiving convulsive therapy for psychiatric illness were studied. Their ages ranged from 19 to 67 years, the median being 40 years. Each patient received at least 12 treatments at a rate of three times weekly with the Medcraft alternating current instrument. Neither barbiturates nor succinylcholine were used as premedications.

An electroencephalogram was taken prior to treatment and during the second, third and fourth weeks of treatment on a non-treatment day. Patients whose pre-treatment records were asymmetric, or manifested slow wave or spike activity were excluded from the study. The EEGs were rated as showing a low, moderate or high degree of delta change based on criteria previously described (4). These criteria include measurements of per cent time delta, slowest frequency, highest voltage and duration and average voltage of burst activity, in selected leads.

The threshold for each seizure was determined as follows. The initial electrical application was 90 or 100 volts at .2 seconds. Voltages were increased by 10 volt steps at 20 second intervals to 140 volts, if a seizure did not result. The duration was next increased to .3 seconds and the voltage was reduced to 100 volts and again increased by 10 volt steps to 140 volts in a similar manner. This procedure was extended to durations of .4, .5 and .6 seconds. The voltage and duration at which a seizure occurred was recorded as the threshold. Stimuli which did not induce a generalized seizure produced loss of consciousness so that the repeated subconvulsive stimuli were not experienced by the patient.

The duration of a seizure was measured from the onset of the tonic phase to the end of the clonic phase. The latency time between the application of the stimulus to the onset of the seizure was not included in this measurement.

RESULTS

The threshold for the initial seizure varied from 90 volts at .2 seconds to 100 volts at .4 seconds. Statistical analysis showed no significant relationship between this threshold and the degree of EEG delta change that subsequently developed during therapy (Table 1).

The change in threshold during treatment was variable. A rise occurred in 21 patients. There was no relationship between the amount of this rise and the degree of EEG change. Fourteen patients showed little or no change in threshold. A larger percentage of "high degree of delta change" records occurred in this group than in the previous group.

TABLE 1

Threshold for Initial Seizure and EEG Change

No. of Patients	Threshold	EEG Change*	
		0-H or 1-H	2-H or 3-H
12	90v 0.02 second	6	6
6	100v 0.02 second	5	1
7	110v 0.02 second	4	3
6	120v 0.02 second	7	0
6	130 or 140v 0.02 second	1	4
2	100 – 120v 0.03 second	1	1

* 0-H or 1-H indicates no or one high degree delta change record during therapy. 2-H or 3-H indicates that two of the three records or all of the records during therapy showed a high degree of delta change.

TABLE 2

Duration of Total Seizure Time in Seconds and EEG Change

	0-H and 1-H	2-H and 3-H	p*
ECT 1 to 3 (Mean time)	109.6 secs.	119.0 secs.	NS
ECT 1 to 5 (Mean time)	176.2 secs.	191.5 secs.	0.02
ECT 1 to 8 (Mean time)	263.9 secs.	309.6 secs.	0.01
ECT 1 to 10 (Mean time)	344.9 secs.	375.6 secs.	0.01

* Significance of mean difference, Mann-Whitney U Test.

The duration of the initial seizure varied from 27 to 65 seconds, while those of subsequent treatments tended to be shorter. The duration of the seizures were added to obtain the total seizure time for treatments 1 to 3, 1 to 5, 1 to 8 and 1 to 10. There was a significant relationship between the mean duration of seizures 1 to 5, 1 to 8 and 1 to 10 and the degree of EEG change. The longer the duration, the greater the degree of EEG change (Table 2). In addition, the (biserial) correlation between seizure time and EEG change was significant. For the 1–3 period, the correlation was +0.34; 1–5 period, +0.52; 1–8 period, +0.51 and 1–10 period, +0.57. There was no relationship between the mean duration of the initial seizure or the mean duration of seizures 1 to 3 and the degree of EEG change.

DISCUSSION

The data presented demonstrate a relationship between the total seizure duration and the degree of EEG change in a group of patients receiving grand mal therapy with alternating current at threshold strength. The longer the convulsions, the greater the degree of EEG change.

This relationship is in agreement with the general observations that unmodified alternating current therapy produces stronger and longer convulsions and a greater degree of EEG change than various modified therapies. For example, therapy with sub-convulsive stimuli produces little or no change in the EEG (5). Similarly, therapy with induced focal rather than grand mal seizures produces less change in the EEG than standard grand mal therapy (1). Finally, it had been our impression that therapy with modified currents such as produced by the Reiter instrument induces a "softer" seizure of shorter duration and, at times, partial

seizures. This is particularly true when amobarbital or thiopental is used as pre-medication, as is the practice (9). With such therapy a greater number of treatments are necessary to induce a significant change in the EEG as compared to conventional alternating-current therapy (14).

It might be proposed that further increasing the duration of seizures would result in even greater EEG change. However, our experience with hexafluorodiethylether (Indoklon) does not substantiate this. Seizures induced by Indoklon are longer than those induced by electrical therapy. Yet, in patients undergoing such therapy, the rate and degree of development of delta activity in serial records were similar to those occurring in patients receiving alternating current therapy.

This variability in duration of seizures and in EEG change and their relationship may be a reflection of a common inherent neurophysiological reactivity, *i.e.* the capacity of the individual nervous system to respond to stimuli or to injury (6). The nature of this reactivity is not known but is probably biochemical and may be related to acetylcholine metabolism (2).

Many investigators have suggested factors associated with individual variability in the ECT threshold (8, 10, 16, 17). Among these are resistance of the scalp and head, age, characteristics of the pre-treatment EEG, blood metabolites and the temperature and humidity of the environment. Although these factors may affect threshold, none appear to be decisive variables. In these studies, head resistance, age and characteristics of the pre-treatment EEG were not related to the initial threshold. Similarly, the present study showed no relationship between threshold and the degree of EEG change during treatment.

It is appreciated that a more significant measurement of the seizure threshold might be a determination of the amount of current applied. However, it was not possible to make such measurements.

Only a small amount of the current used for treatment reaches the brain (7, 15). Most of the current is shunted into the extracerebral tissues and this probably varies considerably between patients. Only with the use of intra-cerebral probes would it be possible to take accurate measurements of the stimulating current and allow for more precise determinations of seizure threshold.

Measurement of seizure threshold in terms of voltage and duration, as done in this study, appears to be a reflection of the actual threshold. The repeat variability of such measurements during a course of treatment is high. Drugs, such as amobarbital, which are known to elevate seizure threshold also elevate the threshold as measured by this method.

Since completion of this study, Ottosson (12) has published results which support the relationship demonstrated here between the duration of seizures and the EEG changes during convulsive therapy. When lidocaine was given as pre-medication the seizures were shorter in duration and the immediate post-seizure EEG showed a "lower degree of functional cerebral disturbance" as compared to unmodified convulsions.

SUMMARY

In 39 patients receiving ECT with alternating current at threshold strength the threshold and duration of seizures were compared to the degree of delta change as analyzed in serial electroencephalograms during the course of treatment.

No relationship was found between the threshold for the initial treatment and the amount of delta activity induced during treatment.

There was a positive correlation between the total duration of the seizures for five, eight and ten treatments and the degree of EEG change during treatment. The longer the duration of seizures, the greater the degree of EEG change.

Both the duration of the seizures and the degree of EEG change may be manifesta-

tions of a common inherent neurophysiological reactivity.

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STUDIES OF BODY IMAGE IN MEN WITH SPINAL CORD INJURY

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INTRODUCTION

One might suspect on theoretical grounds that the patient with a spinal cord injury would have considerable disturbance of body image. The sudden loss of lower limb function, bladder, bowel and sexual function that occurs with traumatic myelopathy would be expected to produce profound alteration in the individual's conception of his body as an integrated whole.

According to Fisher and Cleveland (3), "body image" is a term which refers to the body as a psychological experience and focuses on the individual's feelings and attitudes toward his own body. It is concerned with the individual's subjective experiences with his body and the manner in which he has organized these experiences. The assumption is that as each individual develops, he has the difficult task of meaningfully organizing the sensations from his body. This organization is considered to be one of the most complex and important tasks in his total perceptual development. The body image is therefore literally an image of his own body which the individual has evolved through experience. Actually, the term "body image" involves no assumptions regarding the availability to conscious knowledge of such attitudes and feelings. Indeed, it is currently a loose, generalized term with few specific connotations.

When using the concept of body image in this sense, it appears that the "Draw-a-Person" test may be well-suited for the study of changes in body image which might attend loss of body structures or functions. The suitability of the Draw-a-Person test for such study is based on the assumptions implicit in the projective hypothesis, which implies that in producing drawings of a person, an individual draws on his own inner experiences and feelings, and projects his particular ideas and conflicts about his own body into his productions of human drawings. Because of this assumption, the Draw-a-Person test has become a popular addition to the usual projective test batteries employed in clinical psychology.

The work of Machover (6) has elucidated the value of the Draw-a-Person test in personality evaluation. As she states, "The drawing of a person involving a projection of the body image provides a natural vehicle for the expression of one's body needs and conflicts. In the reproduction of a drawing, there emerges out of the individual's total experiental background a unique pattern of movement and idea. Its significance for personality stems from the fact that there are involved processes of selection of the infinite pool of experience and imagery, potentially available in combination with a dynamic organization of movement and percepts."

The interest of the neurologist in body image grows naturally out of his everyday work with patients who manifest changes in the functioning of their bodies. It is logical for the neurologist to wonder how the patient subjectively perceives such changes as occur in catastrophic illness such as hemiplegia, spinal cord injury, etc. Particularly, he is interested in whether or not major neurological dysfunctions affect the body image and, if so, in what specific ways.

Around the turn of the century, interest in body orientation distortions was apparent among European neurologists. In 1922, the German neurologist, Pick, described distinct

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types of disturbances related to the individual's orientation toward his own body (10). He discussed such problems as inability to distinguish the right side of the body from the left, phantom limbs, and other defects in perception of the body structures, and attempted to relate these to some central representational mechanism akin to the body image concept. At about the same time, Henry Head (4, 5) similarly stressed the relationship of certain neurologic symptoms to disturbances in judgments about one's body. He assumed this to be related to a faulty general standard or *schema* against which a patient may be judging his own movements and postures. Schilder (12, 13) and others in Vienna became interested in the relationship of neurological disorders to body image. Schilder and his co-workers found a large variety of body image distortions in patients with brain damage, and attempted to link body image abnormalities to cerebral lesions discovered at autopsy. They found evidence that parietal lobe lesions were most frequently present in patients with body image pathology. However, these relationships have been more recently modified by the work of Critchley (1, 2).

Schilder has stressed the "enormous psychological importance of all openings of the body," since it is by these openings that contact is made with the outer world throughout life. It is with the body openings that the greatest emotional needs of the infant, as well as of the adult, are satisfied. Historically, the phallus was a symbol of strength, power, manliness, and even deity (11). Although phallic worship is no longer practiced in western civilization as in ancient times, symbolic significance of the phallus as the source of masculinity persists. A man's feelings of adequacy depend in some definite degree on his self-appreciation of sexual integrity and potency.

Thom *et al.* (16) have stated that the paraplegic's ego suffers a great blow at the time of injury. The realization of the loss of normal sexual, bowel and bladder function is met, in their opinion, with a profound ego disturbance. They found that most patients in this situation become depressed, overdependent, and unrealistic about the future, with much fantasy and autistic thinking. Nagler (8) has stated that the paraplegic suffers a loss of prestige. He whose loss is only functional has greater difficulty in facing reality than does the amputee, whose loss is structural, according to this author. Mosak (7) studied 25 paraplegic and 25 non-paraplegic veterans with the Harrower-Erikson multiple choice test and, in addition, analyzed their drawings of a male and a female figure, as to the expression of movement of the lower extremities. He found that only six of the 25 paraplegics drew functional legs, and that many drew only the head of the figure. Seymour (14) stated that those paraplegics who are demanding and outwardly aggressive are struggling to maintain the integrity of the ego, whereas those patients who have become passive and autistic have given up the fight to save the ego integrity and have entered a situation of ego functioning on a lower level. All this suggests that the patient with a spinal cord injury is faced with a severe psychological loss which tends to undermine his self-esteem and self-concept as an integrated individual.

In contrast to these studies suggesting considerable psychological disturbance in paraplegics, Petrus and Balaban (9) have found that there is no increased incidence of psychosis among paraplegics. In fact, even neuroses are uncommon. Rather, the paraplegics that they studied became indifferent, passively aggressive, extremely dependent, or showed sociopathic behavior. However, these changes may indicate reestablishment of new attitudes associated with disturbances of the body image.

The purpose of this study is to evaluate the self-concept in a group of males with spinal cord injury, all of whom have disturbances of bowel, bladder, sexual, and

lower extremity functions, by means of the Draw-a-Person test.

PROCEDURE

Two groups of in-patients at the Veterans Administration Hospital, Hines, Illinois, were selected for study. The experimental group consisted of 30 males with traumatic myelopathy. There were four patients with cervical cord lesions, 24 with thoracic cord lesions, and two with lumbar lesions. Patients were selected at random from the spinal cord injury service, the only requisite for inclusion in the group being the ability to use an arm for drawing. The inability of most quadriplegics to draw resulted in few patients with cervical level injury in the series. All patients had impairment of bowel, bladder and sexual functions. Five had automatic bladders. The remainder had either indwelling urethral or suprapubic catheters. Bowel function in all patients was one of manual removal of stool or via manual anal stimulation with reflex defecation. In none of the patients was erection with genital sensation present. Thirteen of the patients were married and nine had children. The patients' ages ranged from 21 to 70, with a mean age of 38 years. Twenty-five patients spent most of their waking hours out of bed and in wheel chairs. The remaining five were also generally out of bed but on litters. All were able to move about by themselves, but none was able to walk. The education of the experimental group ranged from five to 18 years, with a mean of 11 years. The duration of injury ranged from four months to 15 years, with a mean of 6.4 years. The mean total length of hospitalization was 5.4 years, with a range of four months to 15 years.

The control group consisted of 30 male in-patients at the same hospital, selected from the medical and medical neurology wards. These members were all ambulatory and had normal bowel, bladder and sexual function. Only patients who had chronic "incurable," but not necessarily fatal, diseases were chosen, however. An effort was made to choose as controls those patients who had been in the hospital for the longest time so as to have a group whose hospital stay corresponded as nearly as possible to that of the experimental group. Alcoholics were eliminated as controls. Members of the control group had the following diagnoses: diabetes mellitus (eight patients); arteriosclerotic heart disease (four patients); Hodgkin's disease (three patients); and one patient each with cervical cord tumor, Meniere's disease, multiple sclerosis, malaria, idiopathic purpura, post-traumatic epilepsy, dermatomyositis, multiple myeloma, Wilson's disease, Guillian-Barré syndrome, idiopathic cerebellar degeneration. Four controls had undiagnosed illnesses. The age range of the controls was from 27 to 70 years, with a mean of 40.4 years.

Members of the control group had educational levels ranging from five to 15 years, with a mean of 10 years. The length of illness of the control group varied from two months to 40 years, with a mean of 4.2 years. The mean total length of hospitalization was 2.5 months, with a range from six days to two years. Thus, in all respects, except length of hospitalization, the experimental and control groups were quite comparable.

All members of both experimental and control groups were tested by the same examiner. The subject was informed that this was not a test and had no bearing on his medical treatment. He was then asked to draw a person. No directions were given as to whether a whole or part of a person was to be drawn, nor was the sex of the drawing specified. Upon completion of the drawing, the subject was asked to identify the sex of the figure drawn. Then the subject was asked to draw a person of the sex opposite from the first. Finally, the subject was asked to tell a story about the male figure, directed in response to the following questions: (a) What kind of person is this man? (b) Is he a dependable person? (c) What

kind of a character does he have? (d) Is he a family man? (e) Does he provide well for his family? (f) Are his family relationships good, or is there a great deal of conflict? (g) What are his plans for the future? (h) Do you think that his future will be secure?

The story was intended to elicit information about (a) adequacy of the figure; (b) the figure's family status; and (c) the future plans and eventual fate of the figure. Each subject was permitted to spend as much time as desired in the testing.

RESULTS

The completed drawings were studied by the following methods:

Total figure ratings. The 60 male drawings were coded, randomized, and then studied by two psychologists trained in the analysis of the Draw-a-Person test, for independent evaluation of the overall abnormality of the male figures. The 30 most pathological figures chosen by each psychologist were then tabulated as to the number of control and experimental group members. Psychologist A rated 13 experimental subjects and 17 controls among the most pathological 30 drawings, while Psychologist B rated 16 experimental subjects and 14 controls among those 30 drawings he believed to be most pathological. The difference between proportions in ratings by the two psychologists was clearly not significant at the .05 level. Among the 30 most pathological drawings, as independently rated by two psychologists, there were approximately equal numbers of spinal cord injury patients and controls. Consequently, there is no evidence that the human drawings of spinal cord injury patients are clinically distinguishable from those of other chronically ill patients used in this study.

Analysis of the figures in terms of certain characteristics of the drawings. This includes comparing the drawings in terms of 24 criteria which have been used extensively in previous figure analyses (17).

These criteria included quantitative measures of length in mm. of total size of the human figure, and also separate measures of length of head, body, lower body, and each arm. Further, presence or absence of whole or part of each extremity, eyes, ears, and body were tabulated. Also, posture, placement, position, tilting of figure, degree of shading, and absence of clothing were tabulated separately. This was done on the assumption that these variables may reflect some psychologically meaningful clues to changes in body image. A previously conducted experimental study by M. S. Zaks and M. H. Groves (17), involving testing of a group of 30 "normal" individuals under usual conditions and under experimentally induced stress, provided interpretive criteria for these variables.

The application of appropriate t-tests and chi-square tests to drawings of the female figure revealed no significant differences between the experimental and control groups, except for the total figure areas, where a minor tendency was noted for the experimental group to draw larger sized figures ($t = 1.4$; $p < .20$). As in the drawings of the female figure, so also in the drawings of the male figure the experimental group's drawings tended to occupy a larger area of the page than did those of the controls ($t = 1.8$; $p < .10$). In addition, members of the experimental group used more shading than did controls in their male drawings. In the experimental group, 15 subjects (50 per cent) shaded their drawings, while only three control subjects (10 per cent) did so. The difference between proportions is significant at a better than .01 level. A further finding was that in the spinal cord injury group, as well as in the control group, the female figure was significantly larger than the male figure (spinal cord patient group: $t = 3.6$; $p = .01$; control group: $t = 6.0$; $p = .001$). It was also found that in the drawing of the male figure, a significantly larger number of spinal cord injury patients (11 patients) drew the figure facing toward the right of the page than

d the controls (three patients). (The test f significance between proportions is sig- ficant: $p < .05$.) Table 1 contains the sig- ficant data for the comparison of control d experimental groups.

First figure drawn. As stated above, the ubject was asked in the test procedure to draw a person," with no direction from the xaminer as to the sex of the drawing. The ubject was subsequently asked to identify his drawing's sex. Twenty-five of the ex- erimental subjects and 28 of the controls rew the male figure first. There was no ignificant difference between the two groups as to the sex of the first-drawn figure.

Evaluation of the story about the male figure. The stories were randomly ordered and then evaluated separately for each of the three criteria by one of the authors. No significant differences were found between the stories of the two groups, when compared for satisfactory family status and an optimistic outlook for the future. There is a tendency for the controls to describe the male figure as being an adequate person, while the experimental group tends to describe him more often as an aimless, irresponsible, rather inadequate person. Male figures are described as inadequate by 13 experimental subjects and six controls. (Difference between proportions resulted in $p < .10$.)

Results of Figure Analysis

	Male Figure			Female Figure		
	Experimental Group	Control Group	p	Experimental Group	Control Group	p
Total figure area—mean (cm.²) (*t*-test)	69.9	45.4	0.20	81.3	51.4	0.10
Shading in drawings (x^2)	15	3	0.01	11	10	N.S.
Figure facing to right of page (x^2)	11	3	0.05	6	2	N.S.

DISCUSSION AND CONCLUSIONS

It is difficult, if not impossible, to provide an ideal control group with which to study patients with spinal cord injury. There is no other group of corresponding age in the general hospital population which compares to the spinal cord injured patients in terms of duration of hospitalization. Non-patient "normals" such as hospital personnel or college students are not suitable controls in a psychological study of patients with spinal cord injury. In this study, the control and experimental groups were of similar composition, *i.e.*, relatively long-term hospitalized male veterans with chronic "incurable" illnesses. It is to be emphasized that although the members of the control group had in all cases organic disease, and in some cases fatal disease, the disease process in all the controls was early in course and/or of mild nature, and caused the patient no impairment of bowel, bladder, sexual or lower extremity functioning.

Our results indicate that global clinical judgment by two psychologists trained in Draw-a-Person analysis failed to identify the spinal cord injury patients. The analysis of 24 separate variables resulted in only a few differences, and even the few were not pronounced. The tendency to occupy a larger area by both male and female drawings of the spinal cord injury patients, as compared to the control subjects, may be related to the former's exaggerated feelings about body size, or an over-drawing of the body as a compensation for illness, or perhaps a wish to be large, to attract and merit the attention of those on whom he is dependent. It may further reflect the presence of a greater degree of emotional frustration, as it was found by Zaks and Groves (17), that under conditions of situational stress, human figures tend to be drawn larger than under normal conditions.

In projective testing, shading is generally considered an expression of anxiety, or at least psychomotor tension. Its somewhat greater prominence in the drawings of the

experimental group may indicate that this injury group tends to be somewhat more agitated or anxious than the controls.

The fact that the spinal cord injury group, as well as the control group, tended to draw larger female figures than male figures, may be related to the great dependence of chronically ill patients on nurses, aides, physical therapists, Red Cross workers, etc., most of whom are female, for the patients' everyday care and well being. It is a common observation that during the initial period following cord injury, the male has reticence in exposing his genitalia to the female nursing personnel in the same degree as does any other male patient. Very rapidly, however, the spinal cord injured patient loses this embarrassment and accepts his dependent role, with few reservations about exposing himself when this is necessary for his care.

The drawing of the figure facing toward the right margin of the page is often clinically ascribed to a high dependence on the environment. However, experimental evidence to support this is lacking.

In view of the fact that 24 criteria were used, the number found to show tendencies for differences between the groups is certainly not impressive. Thus, the data from this study utilizing the Draw-a-Person test suggest that while the patient with a spinal cord injury tends to have a somewhat greater degree of anxiety or psychomotor tension and tends to be more dependent on his environment, especially on the female figure, his body image differs in no other significant way from that of other chronically ill, relatively long-term hospitalized men with intact bowel, bladder, sexual and lower extremity functions.

In this study, non-patient normals were not used as a basis for comparison with spinal cord injury patients. On the basis of previous study of the Draw-a-Person test in non-patient normals, it appears that there are many abnormalities in the drawings of our experimental as well as our control group, when compared to such normals. For example, the fact that both the experimental and control groups drew significantly larger females than males is pathological, as compared to findings in "normal" males. It was found that under non-threatening conditions, male and female drawings do not differ in size; but under conditions of experimentally induced frustration or stress, the male figure is drawn significantly larger than the female figure (17). However, a statistical comparison involving non-patient normals was not made in this study.

SUMMARY

The male and female drawings of 30 men with spinal cord injury were analyzed and compared with the drawings of 30 controls. The control group was composed of chronically ill, relatively long-term hospitalized male veterans comparable to the experimental group with respect to age, education, and length of illness.

The patients with spinal cord injury showed a tendency toward more anxiety or tension, and greater dependence on their environment, especially on the female figure in this environment, than did the controls.

In most respects, there was no significant difference between men with spinal cord injury and controls, in body image, as determined by the "Draw-a-Person" test. The failure to find pronounced differences between these two groups may be a reflection of the general degree of disturbance in psychological functioning of any chronically ill patient.

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THE USE OF SOCIOLOGICAL AND SOCIAL-PSYCHOLOGICAL CONCEPTS IN PHYSIOLOGICAL RESEARCH: A REVIEW OF SELECTED EXPERIMENTAL STUDIES

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Recently, along with social psychology and psychophysiology, a relatively new area of interdisciplinary research has been receiving increasing attention from behavioral scientists. This field has been called "sociophysiology" (2) or "interpersonal physiology" (5)—the study of the reciprocal influence of human physiological systems and social systems.

Interdisciplinary research can only proceed insofar as common concepts are accepted and utilized by members of the cooperating disciplines. In some instances the commonly accepted concepts are introduced at the stage of the research design. These studies are interdisciplinary by intent. On other occasions, such concepts are introduced only in order to interpret certain findings. These studies are interdisciplinary by accident. In both situations, however, the cause of interdisciplinary cooperation is forwarded.

It is our purpose to review several selected experimental studies in which sociological and social-psychological concepts are employed in physiological research. The studies are organized according to what we construe to be their appropriate social-psychological or sociological concepts. These concepts have been chosen because of their traditional usage in sociological literature or the interest they have evoked, at least by implication, from physiologists. Since, very often, different terms have been used to denote substantially similar concepts, we have taken the liberty of grouping the studies under what we judge to be the most suitable traditional term. Resulting are the following four concepts, each of which is discussed separately: social status, social sanction, definition of the situation, and empathy.

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SOCIAL STATUS

Social status is defined here as the social position that an individual occupies relative to other social positions, which serves as the focus for a complex of normative expectations incumbent upon the actor when interacting with specified relevant others.

This concept has been employed as a variable in at least two sociophysiological studies.[2]

In one study, Rankin and Campbell (10), interested in studying the effects of aspects of the experimental situation on physiological responses, used the psychogalvanometer to measure differential responses of subjects to physical contact with Negro and white experimenters. Two scales of attitudes toward Negroes were also administered. In general, the results demonstrated a positive relationship between the galvanic skin reflex (GSR) and attitude toward Negroes; that is, persons with greater prejudice toward Negroes showed increased GSR upon contact with a Negro.

The concept of social status, by implication, was also used by Reiser and associates (11) in their investigation of the effect of variations in laboratory procedure and experimenter upon physiological functions. In this connection, they suggested that the development of methods for the evaluation of the effects of such variations "might well make it possible to study some aspects of interpersonal relations (and their physio-

[2] These studies serve as examples of those using other than the traditional terms for substantially similar concepts.

gical concomitants) as they occur naturally, rather than through the use of artificial devices such as the superimposition of unusual and often incompletely understood 'stress'" (11, p. 186).

To this end, two experiments on comparable subjects (healthy young soldiers) were carried out. Half the subjects were tested by a captain-psychiatrist (Experiment 1), the other half by a private-physiologists[3] (Experiment 2). In each experiment, half the subjects (control group) were immediately reassured that the experimental procedure was harmless, but half (experimental subgroup) were not given this information until later.

In the results of each experiment, there were significant differences between the control and experimental subgroups in the mean physiological response to the interview. However, in each experiment the differentiation between subgroups was related to only one of the several physiological functions; and the one function varied with the status of the experimenter. In the first experiment, carried out by the officer, the two subgroups were differentiated only by amplitude of the ballistocardiogram. In the second experiment, executed by the enlisted man, the two subgroups were distinguished from each other only by mean arterial blood pressure. Thus the nature of the physiological response of the subjects varied with the status of the person with whom they interacted.

The interview data suggested that "the main emotional change during the experimental interviews in Experiment 1 was relief of uneasiness and tension, whereas the main emotional shift distinguishing the experimental subjects in Experiment 2 appeared to be the discharge or release of angry, resentful feelings" (11, p. 198) which were presumably inhibited, in the first experiment, due to the presence of the officer.

Aside from the obvious contribution of lending greater credibility to the statement that social behavior has physiological consequences, such research may make a contribution to the construction of an empirical typology of social relationships. The two studies which we have discussed were alike in their use of social status as an independent variable. However, the research of Rankin and Campbell differed from that of Reiser *et al.*, in several respects. One important difference was the nature of the physiological variables used; but another difference which is possibly of greater interest to the sociologist concerns the nature of the particular social relationships under study. For example, the Negro-white relationship differs along several dimensions from that of the officer-enlisted man relationship, such as the degree of formality, the permissibility of demonstration of affect, the nature of the sanctions employed, and the basis of the power allocation. Such differences suggest the possibility of systematically varying interpersonal situations, and at the same time investigating variation in physiological behavior. Depending upon the special interest of the investigators and the availability of conceptual apparatus, such research may be conducted in one of two ways.[4]

First, assuming the lack of an adequate typology of social relationships, a population of subjects may be tested in a variety of social relationships in which the subjects normally find themselves by means of a battery of physiological tests. An examination of the results might reveal the existence of homogeneous physiological behavior in certain of the relationships as opposed to others. In such a manner the experimenter might be able to isolate clusters of social relationships on the basis of like physio-

[3] The "status" variable was a complex one in that the experimenters differed as to military rank, professional status, experience, and personality. However, supplementary psychological data indicated that military rank was the primary determining factor.

[4] In either procedure the experimenter would of necessity control on the factor of individual differences in physiological response.

logical responses, thus providing a possible foundation for an empirical typology of social relationships.

The second possibility demands the prior existence of a typology of social relationships such as the Parsonian schema of pattern variables (9) on the assumption, of course, that such variables can be operationally defined. Once having developed a small number of well defined dimensions, we may derive the finite number of combinations and permutations thereof, and then gather physiological data for subjects engaged in each of the varieties of social relationships. For example, we might apply a battery of physiological tests to subjects engaged in social relationships characterized by a performance—specific—universalistic—affectively neutral orientation as opposed to social relationships characterized by a qualitative—diffuse—particularistic—affective orientation (9). Such data might lend support to the initial typology and might provide the physiologist with data of strategic importance.

In each of the two proposed approaches, the methodology permits the use of several techniques depending on specific limiting circumstances such as the nature of the physiological measures. Thus the subjects' behavior in a variety of social relationships could be gauged by: observation in actual life situations; the use of role-playing techniques; hypnotic suggestion; or, perhaps, simply allowing the subject to imagine himself acting in a variety of life situations.

In any case, it is not our purpose to discuss the practicality of any specific research design but, rather, merely to suggest that the use of the concept of social status within a physiological framework might contribute to sociological theory, physiological theory, and to a more holistic theory of human behavior.

SOCIAL SANCTION

While the phenomenon of social sanction is present in all social interaction it has been systematically studied as a variable in relatively few experimental sociophysiological studies.

Social sanction is defined here as behavior by the subject that serves, either by intent of subject and/or perception by the social object, as a reward or punishment for behavior by the social object.

One example of the utilization of this concept as a variable is the Malmo, Boag, and Smith study (8) of the interaction between interviewer and interviewee by physiological recording techniques. While the experimenters did not employ the term "sanction," their operational definition clearly conforms to the requirements of our definition.

Nineteen female psychoneurotic patients were asked to relate a story in response to a Thematic Apperception Test (TAT) card. The examiner then either praised or criticized the patients' stories. Among the results was the finding that, during the rest intervals following the praise or criticism, the speech muscle tension fell rapidly following praise in contrast to the non-falling tension following criticism. Thus, differential sanctions were accompanied by differential physiological responses.

The employment of the concept of sanction within a sociophysiological context suggests a variety of investigations which might be fruitful for both the sociologist and the physiologist. For example, it might be used to investigate the operation of the "reference group" phenomenon. By measuring the physiological responsiveness of a subject to the sanctions applied by representatives of a variety of groups, it may be possible to determine the degree to which each group serves as a reference group for the subject.

Physiological response might serve also as a clue to the relative effectiveness of different sanctions applied by the same individual; or, it might permit the investigation of the optimal sanction(s) for each of several statuses. What is an effective sanction when applied by an occupant of one status

is not necessarily effective when applied by the occupant of a different, though comparable, status.

It appears, then, that this concept is another which might be profitably utilized as a common concept by the sociologist and the physiologist. Often the responses of one individual to the sanctions of another are primarily or entirely covert. When this is the case, the use of physiological recording techniques assumes an even greater importance.

DEFINITION OF THE SITUATION

Several studies, either in the research design or in the interpretation of results, discussed phenomena that are closely related to the traditional sociological concept, "definition of the situation." This concept is defined here in terms of the relative clarity of the normative expectations that are applicable to the individuals in a social situation.

Boyd and DiMascio (2) found in their study of the psychotherapeutic interview, that as the particular interview under study proceeded, the heart rate became slower, sweating increased, and face temperature increased. These findings suggested to the investigators that the interview produced an immediate increase in sympathetic nervous tension, but as time passed and the patient apparently *adapted socially* to the psychiatrist, sympathetic relaxation occurred.

This investigation is based on the study of only one patient in therapy, but its results are suggestive especially in the light of other related studies.

It will be recalled that Reiser *et al.* (11) performed two experiments on healthy young soldiers, half of whom were tested by a Captain (Experiment 1) and half of whom were tested by an enlisted man (Experiment 2). However, they also introduced another variable—*relative uncertainty as to the exact nature of the procedure.* In each of the two experiments, half the subjects (control subgroup) were immediately "reassured as to the benign nature of the procedure" (11, p. 198), and half (experimental subgroup) were not given this information until later.

In each case, the subgroups were distinguished on the basis of a statistically significant difference in the mean physiological response, indicating that variation in the definition of the situation may lead to variation of physiological response.

Another sociophysiological project, that conducted by Lennard *et al.* (6), investigated the patterns of interpersonal communication of a small group of individuals under the influence of the hallucinogenic drug LSD-25 (lysergic acid diethylamide). Four-person groups were studied under several conditions, varying placebo and drug. The social interaction was described primarily in terms of Bales' interactional categories (1).

Among the results were the findings that the ratio of orientation to evaluative responses, and the ratio of questions to answers was higher in the group under "condition LSD-25" than under condition "normal." These results were interpreted by the experimenters "as an attempt on the part of the group to restore cognitive clarity despite the felt impairment" (6, p. 198).

In spite of the differences in the terminology employed in the studies discussed above, "social adaptation," "relative uncertainty as to the exact nature of procedure," and "degree of cognitive clarity" are clearly related to the concept of "definition of the situation" as we have defined it. This appears to be another area in which sociologists and physiologists might profitably cooperate.

The physiologist might find it useful, for example, to systematically investigate the manner in which physiological functions vary in response to varying degrees of clarity of normative expectations—from highly structured situations to almost anomalous situations. Similarly he might consider the possibility of the similarity of, or differentiation between, physiological behavior in response to vaguely defined situations as

opposed to clearly defined but conflicting situations.

Psychophysiologists recently have been concerned with the physiological concomitants of "stress" (7). The use of concepts such as "definition of the situation" might aid them in integrating their findings, and sensitize them to the great variety of stress-inducing situations present in social interaction.

EMPATHY

In the same way that the concepts of status, sanction, and definition of the situation were utilized in physiological research, by whatever terms they were known, so did the idea of "empathy" find its way into this area of investigation. Perhaps more so in this case than in any of the others, the concept tended to be introduced only at the stage of interpretation of the results.

For present purposes, empathy may be defined as the ability to subjectively respond (at the cognitive, affective and conative levels) in the same way to the same stimulus field as another individual responds.

The findings in at least three studies suggest the possible utility of a physiological orientation in the investigation of empathic phenomena.

We have already referred to the study of Malmo and associates (8) in another connection. In this study, it will be recalled that the examiner either praised or criticized the stories related by nineteen female psychoneurotic patients in response to a TAT card. The patients' speech-muscle tension tended to fall rapidly following praise but remained high following criticism. This same pattern of physiological reaction was manifested by the examiner. His speech-muscle tension fell after he had praised the patients' stories and remained high following criticism of the stories. Furthermore, an investigation of the relationship between the examiner's feeling states (as manifested in his diary notes) and the patient's physiological reaction during the TAT revealed that "on the examiner's 'bad' days, the patient's mean heart rate rose significantly more than on the examiner's 'good' days" (8, p. 119).

Other evidence of the physiological relationship existing between individuals in social interaction was provided by Coleman, Greenblatt, and Solomon (3) who collected physiological and psychiatric data during forty-four interviews for a patient-therapist relationship. An analysis of the patient's heart rate and emotional expression indicated that anxiety was associated with the highest heart rate, depression with the lowest, and hostility in between. A possible physiological indentification of the therapist with the patient is suggested since the therapist's heart rate response to the patient's emotional expression produced the same pattern as that of the patient with the one exception that anxiety demonstrated no difference from extra-punitive hostility. While the therapist's physiological reaction to the patient's behavior varied somewhat from session to session, possibly due to disturbance by "extra-therapeutic concerns or by negative countertransference feelings" (3, p. 8), the investigators suggested that the general similarity of the therapist's physiological reaction to the patient and the patient's own physiological reaction was directly involved in the empathic process.

The same forty-four interviews, with a paranoid-schizophrenic patient, enabled Di Mascio, Boyd, and Greenblatt (5) to study the synchronous recordings made of social interaction and physiological activity. The observations of social interaction were treated primarily in terms of Bales' interaction process analysis (1).

When the patient manifested "tension" his heart rate tended to be higher as opposed to "tension release" where the heart rate tended to be lower. There was a tendency for the heart rate of the therapist to follow a similar pattern. On the other hand, when

e patient manifested "antagonism" to- ard the therapist, the patient's heart rate ould slow down whereas the therapist's art rate would be faster. These findings ere interpreted by the investigators as dicating a positive physiological identifi- tion of therapist with patient when the tter was expressing tension or tension re- ase, but a negative one when the patient as expressing antagonism toward the ther- pist. In the former case, we might say that he therapist was acting as if he were the atient while in the latter case the thera- ist was reacting *qua* the object of aggres- ion.

These findings as well as those of the other studies discussed in this section suggest the advisability of reconsidering the concept of empathy within a physiological context. An example of relatively current treatments of the empathic process is contained in this discussion of the role of empathy in the therapeutic situation.

> "This [the therapist's correct perception of the patient's definition of the situation] requires that the psychiatrist utilize some portion of his reactive system to take the role of his patient—to place himself in the psychological shoes of the patient—and perceive the situation from that perspective; and to respond to himself as the patient responds to him. The ability to do this we shall call empathic ability" (4).

While the possible physiological basis of empathy is not denied, it is usually ignored. However, a consideration of this dimension might have important theoretical and methodological consequences. If it could be demonstrated that the empathic process involves a person placing himself in the "physiological shoes" as well as the "psychological shoes" of another person, investigation of the conditions affecting empathic phenomena could be greatly facilitated. It would be practicable to take simultaneous physiological recordings of wo individuals, one reacting to a stimulus field (for example, being shamed), the other presumably empathizing with the first. Such an experimental situation would permit the study of a large number of variables affecting the empathic process, such as the degree of experience with the stimulus field and the nature of the social relationship existing between the actor and the others in the situation. This type of design would permit the researcher to distinguish between those relationships in which there is merely an "intellectual" identification with the other and those relationships in which there is an affective and, presumably, physiological identification.

SUMMARY

In recent years sociological and social-psychological concepts have been applied in physiological studies at an ever increasing rate. The acceptance and utilization of such concepts have been said to form the basis of a relatively new field of inquiry that some investigators have termed "sociophysiology" or "interpersonal physiology." A consideration of several selected experimental studies, in which concepts akin to social status, social sanction, definition of the situation, and empathy have been applied in a physiological context, has suggested the appropriateness of such terms.

However, the review of these studies has also pointed to the fundamental fact that such investigations are interdisciplinary often only by chance rather than by intent; and therein lies a basic weakness. The concept of interdisciplinary activity suggests the need for cooperation by *representatives* of the fields in question as a condition for the common acceptance and utilization of concepts. Yet, while concepts traditionally appropriate to one discipline have been applied in the other, there have been relatively few examples of research involving a team constituted of sociologists and physiologists. With few exceptions, the investigators involved in these studies have had little formal training in the social sciences.

As a result of this situation we find that very often the sociological concepts have been introduced only at the stage of interpretation of findings, and even then these concepts are sometimes naively interpreted. Had these ideas been considered at the stage of the research design, perhaps the results could have been predicted thus lending them greater credibility.

However, even this limited application of sociological concepts in a physiological context has provided data of great interest to both fields and has suggested new areas of inquiry and modes of procedure.

The relationship between social behavior and intra-psychic processes has been well documented, as has the relationship between the latter and physiological activity. However, the conclusion of this "syllogism," the relationship between social behavior and physiological processes, has not been sufficiently emphasized heretofore. This is not to assert that this relationship is not mediated by psychological processes, but rather that for certain purposes the relationship between the social and physiological levels, holding the psychological dimension constant, may be directly investigated thus providing data of value to both of the cooperating disciplines, physiology and sociology.

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PSYCHOANALYTIC "EVIDENCE": A CRITIQUE BASED ON FREUD'S CASE OF LITTLE HANS

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Beginning with Wohlgemuth's trenchant monograph (13), the factual and logical bases of psychoanalytic theory have been the subject of a considerable number of criticisms. These have generally been dismissed by psychoanalysts, at least partly on the ground that the critics are oblivious of the "wealth of detail" provided by the individual case. One way to examine the soundness of the analysts' position is to study fully-reported cases that they themselves regard as having contributed significantly to their theories. We have undertaken to do this, and have chosen as our subject matter one of Freud's most famous cases, given in such detail that the events of a few months occupy 140 pages of the *Collected Papers*.

In 1909, Freud published "The Analysis of a Phobia in a Five-year old Boy" (2). This case is commonly referred to as "The case of Little Hans." Ernest Jones, in his biography of Freud, points out that it was "the first published account of a child analysis" (8, p. 289), and states that "the brilliant success of child analysis" since then was "indeed inaugurated by the study of this very case" (8, p. 292). The case also has special significance in the development of psychoanalytic theory because Freud believed himself to have found in it "a more direct and less roundabout proof" of some fundamental psychoanalytic theorems (p. 150). In particular, he thought that it provided a direct demonstration of the essential role of sexual urges in the development of phobias. He felt his position to have been greatly strengthened by this case and two generations of analysts have referred to the evidence of Little Hans as a basic substantiation of psychoanalytic theories (*e.g.*, 1, 5, 7). As an example, Glover (5, p. 76) may be quoted.

> "In its time the analysis of Little Hans was a remarkable achievement and the story of the analysis constitutes one of the most valued records in psychoanalytical archives. Our concepts of phobia formation, of the positive Oedipus complex, of ambivalence, castration anxiety and repression, to mention but a few, were greatly reinforced and amplified as the result of this analysis."

In this paper we shall re-examine this case history and assess the evidence presented. We shall show that although there are manifestations of sexual behavior on the part of Hans, there is no scientifically acceptable evidence showing any connection between this behavior and the child's phobia for horses; that the assertion of such connection is pure assumption; that the elaborate discussions that follow from it are pure speculation; and that the case affords no factual support for any of the concepts listed by Glover above. Our examination of this case exposes in considerable detail patterns of thinking and attitudes to evidence that are well-nigh universal among psychoanalysts. It suggests the need for more careful scrutiny of the bases of psychoanalytic "discoveries" than has been customary; and we hope it will prompt psychologists to make similar critical examinations of basic psychoanalytic writings.

The case material on which Freud's analysis is based was collected by Little Hans's father, who kept Freud informed of developments by regular written reports. The father

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also had several consultations with Freud concerning Little Hans's phobia. During the analysis, Freud himself saw the little boy only once.

The following are the most relevant facts noted of Hans's earlier life. At the age of three, he showed "a quite peculiarly lively interest in that portion of his body which he used to describe as his widdler." When he was three and a half, his mother found him with his hand to his penis. She threatened him in these words, "If you do that, I shall send for Dr. A. to cut off your widdler. And then what will you widdle with?" Hans replied, "With my bottom." Numerous further remarks concerning widdlers in animals and humans were made by Hans between the ages of three and four, including questions directed at his mother and father asking them if they also had widdlers. Freud attaches importance to the following exchange between Hans and his mother. Hans was "looking on intently while his mother undressed."

Mother: "What are you staring like that for?"

Hans: "I was only looking to see if you'd got a widdler, too."

Mother: "Of course. Didn't you know that?"

Hans: "No, I thought you were so big you'd have a widdler like a horse."

When Hans was three and a half his sister was born. The baby was delivered at home and Hans heard his mother "coughing," observed the appearance of the doctor and was called into the bedroom after the birth. Hans was initially "very jealous of the new arrival" but within six months his jealousy faded and was replaced by "brotherly affection." When Hans was four he discovered a seven-year old girl in the neighborhood and spent many hours awaiting her return from school. The father commented that "the violence with which this 'long-range love' came over him was to be explained by his having no play-fellows of either sex." At this period also, "he was constantly putting his arms round" his visiting boy cousin, aged five, and was once heard saying, "I *am* so fond of you" when giving his cousin "one of these tender embraces." Freud speaks of this as the "first trace of homosexuality."

At the age of four and a half, Hans went with his parents to Gmunden for the summer holidays. On holiday Hans had numerous playmates including Mariedl, a fourteen year old girl. One evening Hans said "I want Mariedl to sleep with me." Freud says that Hans's wish was an expression of his desire to have Mariedl as part of his family. Hans's parents occasionally took him into their bed and Freud claims that, "there can be no doubt that lying beside them had aroused erotic feelings in him;[3] so that his wish to sleep with Mariedl had an erotic sense as well."

Another incident during the summer holidays is given considerable importance by Freud, who refers to it as Hans's attempt to seduce his mother. It must be quoted here in full.

> "Hans, four and a quarter.[4] This morning Hans was given his usual daily bath by his mother and afterwards dried and powdered. As his mother was powdering round his penis and taking care not to touch it, Hans said "Why don't you put your finger there?"
>
> Mother: "Because that'd be piggish."
>
> Hans: "What's that? Piggish? Why?"
>
> Mother: "Because it's not proper."
>
> Hans (laughing): "But it's great fun."

Another occurrence prior to the onset of his phobia was that when Hans, aged four and a half, laughed while watching his sister being bathed and was asked why he was laughing, he replied, "I'm laughing at Hanna's widdler." "Why?" "Because her widdler's so lovely." The father's comment

[3] This is nothing but surmise—yet Freud asserts "there can be no doubt" about it.

[4] Earlier his age during the summer holidays is given as four and a half. Unfortunately, there is no direct statement as to the length of the holiday.

i-. "Of course his answer was a disingenuous o e. In reality her widdler seemed to him f nny. Moreover, this is the first time he l s recognized in this way the distinction l tween male and female genitals instead of enying it."

In early January, 1908, the father wrote t Freud that Hans had developed "a ner- ous disorder." The symptoms he reported ere: fear of going into the streets; depres- on in the evening, and a fear that a horse would bite him in the street. Hans's father suggested that "the ground was prepared by sexual over-excitation due to his mother's tenderness" and that the fear of the horse "seems somehow to be connected with his having been frightened by a large penis." The first signs appeared on January 7th, when Hans was being taken to the park by his nursemaid as usual. He started crying and said he wanted to "coax" (caress) with his mother. At home "he was asked why he had refused to go any further and had cried, but he would not say." The following day, after hesitation and crying, he went out with his mother. Returning home Hans said ("after much internal struggling"), "*I was afraid a horse would bite me*" (original italics). As on the previous day, Hans showed fear in the evening and asked to be "coaxed." He is also reported as saying, "I know I shall have to go for a walk again tomorrow," and "The horse'll come into the room." On the same day he was asked by his mother if he put his hand to his widdler. He replied in the affirmative. The following day his mother warned him to refrain from doing this.

At this point in the narrative, Freud provided an interpretation of Hans's behavior and consequently arranged with the boy's father "that he should tell the boy that all this nonsense about horses was a piece of nonsense and nothing more. The truth was, his father was to say, that he was very fond of his mother and wanted to be taken into her bed. The reason he was afraid of horses now was that he had taken so much interest in their widdlers." Freud also suggested giving Hans some sexual enlightenment and telling him that females "had no widdler at all."[5]

"After Hans had been enlightened there followed a fairly quiet period." After an attack of influenza which kept him in bed for two weeks, the phobia got worse. He then had his tonsils out and was indoors for a further week. The phobia became "very much worse."

During March, 1908, after his physical illnesses had been cured, Hans apparently had many talks with his father about the phobia. On March 1, his father again told Hans that horses do not bite. Hans replied that white horses bite and related that while at Gmunden he had heard and seen Lizzi (a playmate) being warned by her father to avoid a white horse lest it bite. The father said to Lizzi, "*Don't put your finger to the white horse*" (original italics). Hans's father's reply to this account given by his son was, "I say, it strikes me it isn't a horse you mean, but a widdler, that one mustn't put one's hand to." Hans answered, "But a widdler doesn't bite." The father: "Perhaps it does, though." Hans then "went on eagerly to try to prove to me that it was a white horse." The following day, in answer to a remark of his father's, Hans said that his phobia was "so bad because I still put my hand to my widdler every night." Freud remarks here that, "Doctor and patient, father and son, were therefore at one in ascribing the chief share in the pathogenesis of Hans's present condition to his habit of onanism." He implies that this unanimity is significant, quite disregarding the father's indoctrination of Hans the previous day.[6]

On March 13, the father told Hans that his fear would disappear if he stopped put-

[5] Incidentally contradicting what Hans's mother had told him earlier (p. 171).

[6] The mere fact that Hans repeats an interpretation he has heard from his father is regarded by Freud as demonstrating the accuracy of the interpretation; even though the child's spontaneous responses noted earlier in the paragraph point clearly in the opposite direction.

ting his hand to his widdler. Hans replied, "But I don't put my hand to my widdler any more." Father: "But you still want to." Hans agreed, "Yes, I do." His father suggested that he should sleep in a sack to prevent him from wanting to touch his widdler. Hans accepted this view and on the following day was much less afraid of horses.

Two days later the father again told Hans that girls and women have no widdlers. "Mummy has none, Anna has none and so on." Hans asked how they managed to widdle and was told "They don't have widdlers like yours. Haven't you noticed already when Hanna was being given her bath." On March 17 Hans reported a phantasy in which he saw his mother naked. On the basis of this phantasy and the conversation related above, Freud concluded that Hans had not accepted the enlightenment given by his father. Freud says, "He regretted that it should be so, and stuck to his former view in phantasy. He may also perhaps have had his reasons for refusing to believe his father at first." Discussing this matter subsequently, Freud says that the "enlightenment" given a short time before to the effect that women really do not possess a widdler was bound to have a shattering effect upon his self-confidence and to have aroused his castration complex. For this reason he resisted the information, and for this reason it had no therapeutic effect.[7]

For reasons of space we shall recount the subsequent events in very brief form. On a visit to the Zoo Hans expressed fear of the giraffe, elephant and all large animals. Hans's father said to him, "Do you know why you're afraid of big animals? Big animals have big widdlers and you're really afraid of big widdlers." This was denied by the boy.

The next event of prominence was a dream (or phantasy) reported by Hans. "In the night there was a big giraffe in the room and a crumpled one; and the big one called out because I took the crumpled one away from it. Then it stopped calling out, and then I sat down on the top of the crumpled one."

After talking to the boy the father reported to Freud that this dream was "a matrimonial scene transposed into giraffe life. He was seized in the night with a longing for his mother, for her caresses, for her genital organ, and came into the room for that reason. The whole thing is a continuation of his fear of horses." The father infers that the dream is related to Hans's habit of occasionally getting into his parents' bed in the face of his father's disapproval. Freud's addition to "the father's penetrating observation" is that sitting down on the crumpled giraffe means taking possession of his mother. Confirmation of this dream interpretation is claimed by reference to an incident which occurred the next day. The father wrote that on leaving the house with Hans he said to his wife. "Goodbye, big giraffe." "Why giraffe?" asked Hans. "Mummy's the big giraffe," replied the father. "Oh, yes," said Hans, "and Hanna's[8] the crumpled giraffe, isn't she?" The father's account continues, "In the train I explained the giraffe phantasy to him, upon which he said 'Yes, that's right,' And when I said to him that I was the big giraffe and that its long neck reminded him of a widdler, he said 'Mummy has a neck like a giraffe too. I saw when she was washing her white neck'."

On March 30, the boy had a short consultation with Freud who reports that de-

[7] It is pertinent at this point to suggest that Hans "resisted" this enlightenment because his mother had told him quite the opposite and his observations of his sister's widdler had not been contradicted. When he was four, Hans had observed that his sister's widdler was "still quite small" (p. 155). When he was four and a half, again while watching his sister being bathed, he observed that she had "a lovely widdler" (p. 164). On neither occasion was he contradicted.

[8] Hans's baby sister, *not* his mother. Again, the more spontaneous response directly contradicts Freud's interpretation. Thus Freud's subsequent comment that Hans only confirmed the interpretation of the two giraffes as his father and mother and not the sexual symbolism, transgresses the facts.

spite all the enlightenment given to Hans, the fear of horses continued undiminished. Hans explained that he was especially bothered "by what horses wear in front of their eyes and the black round their mouths." This latter detail Freud interpreted as meaning a moustache. "I asked him whether he meant a moustache," and then, "disclosed to him that he was afraid of his father precisely because he was so fond of his mother." Freud pointed out that this was a groundless fear. On April 2, the father was able to report "the first real improvement." The next day Hans, in answer to his father's inquiry, explained that he came into his father's bed when he was frightened. In the next few days further details of Hans's fear were elaborated. He told his father that he was most scared of horses with "a thing on their mouths," that he was scared lest the horses fall, and that he was most scared of horse-drawn buses.

Hans: "I'm most afraid too when a bus comes along."

Father: "Why? Because it's so big?"

Hans: "No. Because once a horse in a bus fell."

Father: "When?"

Hans then recounted such an incident. This was later confirmed by his mother.

Father: "What did you think when the horse fell down?"

Hans: "Now it will always be like this. All horses in buses'll fall down."

Father: "In all buses?"

Hans: "Yes. And in furniture vans too. Not often in furniture vans."

Father: "You had your nonsense already at that time?"

Hans: "*No* (italics added). I only got it then. When the horse in the bus fell down, it gave me such a fright really: That was when I got the nonsense."

The father adds that, "all of this was confirmed by my wife, as well as the fact that *the anxiety broke out immediately afterwards*" (italics added).

Hans's father continued probing for a meaning of the black thing around the horses' mouths. Hans said it looked like a muzzle but his father had never seen such a horse "although Hans asseverates that such horses do exist."[9] He continues, "I suspect that some part of the horse's bridle really reminded him of a moustache and that after I alluded to this the fear disappeared." A day later Hans observing his father stripped to the waist said, "Daddy you are lovely! You're so white."

Father: "Yes. Like a white horse."

Hans: "The only black thing's your moustache. Or perhaps it's a black muzzle."[10]

Further details about the horse that fell were also elicited from Hans. He said there were actually two horses pulling the bus and that they were both black and "very big and fat." Hans's father again asked about the boy's thoughts when the horse fell.

Father: "When the horse fell down, did you think of your daddy?"[11]

Hans: "Perhaps. Yes. It's possible."

For several days after these talks about horses Hans's interests, as indicated by the father's reports "centered upon lumf (feces) and widdle, but we cannot tell why." Freud comments that at this point "the analysis began to be obscure and uncertain."

On April 11 Hans related this phantasy. "I was in the bath[12] and then the plumber came and unscrewed it.[13] Then he took a

[9] Six days later (p. 211) the father reports, "I was at last able to establish the fact that it was a horse with a leather muzzle."

[10] A good example of the success of indoctrination.

[11] One of many leading questions, the positive answer to which of course proves nothing. It is worth noticing how the same question, differently phrased, elicits contrasting answers from Hans. When asked earlier what he thought of when the horse fell, Hans replied that he thought it would always happen in future.

[12] "Hans's mother gives him his bath" (Father's note).

[13] "To take it away to be repaired" (Father's note).

big borer and stuck it into my stomach." Hans's father translated this phantasy as follows: "I was in bed with Mamma. Then Pappa came and drove me away. With his big penis he pushed me out of my place by Mamma."

The remainder of the case history material, until Hans's recovery from the phobia early in May, is concerned with the lumf theme and Hans's feelings towards his parents and sister. It can be stated immediately that as corroboration for Freud's theories all of this remaining material is unsatisfactory. For the most part it consists of the father expounding theories to a boy who occasionally agrees and occasionally disagrees. The following two examples (pp. 209 and 214) illustrate the nature of most of this latter information.

Hans and his father were discussing the boy's slight fear of falling when in the big bath.

Father: "But Mamma bathes you in it. Are you afraid of Mamma dropping you in the water?"

Hans: "I am afraid of her letting go and my head going in."

Father: "But you know Mummy's fond of you and won't let you go."

Hans: "I only just thought it."

Father: "Why?"

Hans: "I don't know at all."

Father: "Perhaps it was because you'd been naughty and thought she didn't love you anymore?"[14]

Hans: "Yes."

Father: "When you were watching Mummy giving Hanna her bath perhaps you wished she would let go of her so that Hanna should fall in?"[14]

Hans: "Yes."

On the following day the father asks, "Are you fond of Hanna?"

Hans: "Oh, yes, very fond."

Father: "Would you rather that Hanna weren't alive or that she were?"

Hans: "I'd rather she weren't alive."

[14] Leading question.

In response to close, direct questioning Hans voiced several complaints about his sister. Then his father proceeded again:

Father: "If you'd rather she weren't alive, you can't be fond of her, at all."

Hans: (assenting[15]) "Hm, well."

Father: "Thats why you thought when Mummy was giving her her bath if only she's let go, Hanna would fall in the water . . . . . ."

Hans: (taking me up) ". . . and die."

Father: "and then you'd be alone with Mummy. A good boy doesn't wish that sort of thing, though."

On April 24, the following conversation was recorded.

Father: "It seems to me that, all the same, you do wish Mummy would have a baby."

Hans: "But I don't want it to happen."

Father: "But you wish for it?"

Hans: "Oh, yes, *wish*."[16]

Father: "Do you know why you wish for it? It's because you'd like to be Daddy."

Hans: "Yes. How does it work?"

Father: "You'd like to be Daddy and married to Mummy; you'd like to be as big as me and have a moustache; and you'd like Mummy to have a baby."

Hans: "And Daddy, when I'm married I'll have only one if I want to, when I'm married to Mummy, and if I don't want a baby, God won't want it either when I'm married.

Father: "Would you like to be married to Mummy?"

Hans: "Oh yes."

[15] A very questionable affirmation.

[16] Original italics suggest a significance that is unwarranted, for the child has been maneuvered into giving an answer contradicting his original one. Note the induced "evidence" as the conversation continues.

THE VALUE OF THE EVIDENCE

Before proceeding to Freud's interpretation of the case, let us examine the value of the evidence presented. First, there is the matter of selection of the material. The greatest attention is naturally paid to material related to psychoanalytic theory and there is a tendency to ignore other facts. The father and mother, we are told by Freud, "were both among my closest adherents." Hans himself was constantly encouraged, directly and indirectly, to relate material of relevance to the psychoanalytic doctrine.

Second, we must assess the value to be placed on the testimony of the father and of Hans. The father's account of Hans's behavior is in several instances suspect. For example, he twice presents his own interpretations of Hans's remarks as observed facts. This is the father's report of a conversation with Hans about the birth of his sister Hanna.

> Father: "What did Hanna look like?"
> Hans (hypocritically): "All white and lovely. So pretty."

On another occasion, despite several clear statements by Hans of his affection for his sister (and also the voicing of complaints about her screaming), the father said to Hans, "If you'd rather she weren't alive, you can't be fond of her at all." Hans (assenting): "Hmn . . well." (See above).

The comment in parenthesis in each of these two extracts is presented as observed fact. A third example has also been quoted above. When Hans observes that Hanna's widdler is "so lovely" the father states that this is a "disingenuous" reply and that "in reality her widdler seemed to him funny." Distortions of this kind are common in the father's reports.

Hans's testimony is for many reasons unreliable. Apart from the numerous lies which he told in the last few weeks of his phobia, Hans gave many inconsistent and occasionally conflicting reports. Most important of all, much of what purports to be Hans's views and feelings is simply the father speaking. Freud himself admits this but attempts to gloss over it. He says, "It is true that during the analysis Hans had to be told many things which he could not say himself, that he had to be presented with thoughts which he had so far shown no signs of possessing and that his attention had to be turned in the direction from which his father was expecting something to come. This detracts from the evidential value of the analysis but the procedure is the same in every case. For a psychoanalysis is not an impartial scientific investigation but a therapeutic measure" (p. 246).[17] To sum this matter up, Hans's testimony is subject not only to "mere suggestion" but contains much material that is not his testimony at all!

From the above discussion it is clear that the "facts of the case" need to be treated with considerable caution and in our own interpretation of Hans's behavior we will attempt to make use only of the testimony of direct observation.

FREUD'S INTERPRETATION

Freud's interpretation of Hans's phobia is that the boy's oedipal conflicts formed the basis of the illness which "burst out" when he underwent "a time of privation and the intensified sexual excitement." Freud says, "These were tendencies in Hans which had already been suppressed and which, so far as we can tell, had never been able to find uninhibited expression: hostile and jealous feelings against his father, and sadistic impulses (premonitions, as it were, of copulation) towards his mother. These early suppressions may perhaps have gone to form the predisposition for his subsequent illness. These aggressive propensities of Hans's found

[17] Nevertheless, both the theory and practice of psychoanalysis are built on these "not . . impartial scientific investigations." For Freud to admit this weakness has some merit, but the admission is neither a substitute for evidence nor a good reason for accepting conclusions without evidence.

no outlet, and as soon as there came a time of privation and of intensified sexual excitement, they tried to break their way out with reinforced strength. It was then that the battle which we call his 'phobia' burst out" (pp. 279–280).

This is the familiar oedipal theory, according to which Hans wished to replace his father "whom he could not help hating as a rival" and then complete the act by "taking possession of his mother." Freud refers for confirmation to the following. "Another symptomatic act, happening as though by accident, involved a confession that he had wished his father dead; for, just at the moment that his father was talking of his death-wish Hans let a horse that he was playing with fall down—knocked it over, in fact" (p. 272). Freud claims that, "Hans was really a little Oedipus who wanted to have his father 'out of the way' to get rid of him, so that he might be alone with his handsome mother and sleep with her" (p. 253). The predisposition to illness provided by the oedipal conflicts are supposed to have formed the basis for "the transformation of his libidinal longing into anxiety." During the summer prior to the onset of the phobia, Hans had experienced "moods of mingled longing and apprehension" and had also been taken into his mother's bed on occasions. Freud says, "We may assume that since then Hans had been in a state of intensified sexual excitement, the object of which was his mother. The intensity of this excitement was shown by his two attempts at seducing his mother (the second of which occurred just before the outbreak of his anxiety); and he found an incidental channel of discharge for it by masturbating . . . . . Whether the sudden exchange of this excitement into anxiety took place spontaneously, or as a result of his mother's rejection of his advances, or owing to the accidental revival of earlier impressions by the 'exciting cause' of his illness . . . this we cannot decide. The fact remains that his sexual excitement suddenly changed into anxiety" (p. 260).[18]

Hans, we are told, "transposed from his father on to the horses." At his sole interview with Hans, Freud told him "that he was afraid of his father because he himself nourished jealous and hostile wishes against him." Freud says of this, "In telling him this, I had partly interpreted his fear of horses for him: the horse must be his father —whom he had good internal reasons for fearing" (p. 264). Freud claims that Hans's fear of the black things on the horses' mouths and the things in front of their eyes was based on moustaches and eye-glasses and had been "directly transposed from his father on to the horses."[19] The horses "had been shown to represent his father."

Freud interprets the agoraphobic element of Hans's phobia thus. "The content of his phobia was such as to impose a very great measure of restriction upon his freedom of movement, and that was its purpose . . After all, Hans's phobia of horses was an obstacle to his going into the street, and could serve as a means of allowing him to stay at home with his beloved mother.[20] In this way, therefore, his affection for his mother triumphantly achieved its aim" (p. 280).

Freud interprets the disappearance of the phobia as being due to the resolution by Hans of his oedipal conflicts by "promoting him (the father) to a marriage with Hans's grandmother . . . instead of killing him." This final interpretation is based on the following conversation between Hans and his father.

On April 30, Hans was playing with his imaginary children.

[18] Thus a theoretical statement, beginning with "We may assume" ends up as a "fact." The only fact is that the assumed sexual excitement is assumed to have changed into anxiety.

[19] But in fact the child was thinking of a muzzle (see above).

[20] It should be noted, however, that Hans's horse-phobia and general agoraphobia were present even when he went out with his mother (p. 167).

Father: "Hullo, are your children still alive? You know quite well a boy can't have any children."

Hans: "I know. I was their Mummy before, *now I'm their Daddy*" (original italics).

Father: "And who's the children's Mummy?"

Hans: "Why, Mummy, and you're their *Grandaddy* (original italics).

Father: "So then you'd like to be as big as me, and be married to Mummy, and then you'd like her to have children."

Hans: "Yes, that's what I'd like, and then my Lainz Grandmamma" (paternal side) "will be their Grannie."

CRITIQUE OF FREUD'S CONCLUSIONS

It is our contention that Freud's view of this case is not supported by the data, either in its particulars or as a whole. The major points that he regards as demonstrated are these: 1) Hans had a sexual desire for his mother, 2) he hated and feared his father and wished to kill him, 3) his sexual excitement and desire for his mother were transformed into anxiety, 4) his fear of horses was symbolic of his fear of his father, 5) the purpose of the illness was to keep near his mother and finally 6) his phobia disappeared because he resolved his oedipus complex.

Let us examine each of these points.

1) That Hans derived satisfaction from his mother and enjoyed her presence we will not even attempt to dispute. But nowhere is there any evidence of his wish to copulate with her. Yet Freud says that, "if matters had lain entirely in my hands . . I should have confirmed his instinctive premonitions, by telling him of the existence of the vagina and of copulation," (see p. 286). The "instinctive premonitions" are referred to as though a matter of fact, though no evidence of their existence is given.

The only seduction incident described (see above) indicates that on *that particular occasion* Hans desired contact of a sexual nature with his mother, albeit a sexual contact of a simple, primitive type. This is not adequate evidence on which to base the claim that Hans had an oedipus complex which implies a sexual desire for the mother, a wish to possess her and to replace the father. The most that can be claimed for this "attempted seduction" is that it provides a small degree of support for the assumption that Hans had a desire for sexual stimulation by some other person (it will be recalled that he often masturbated). Even if it is assumed that stimulation provided by his mother was especially desired, the two other features of an Oedipus complex (a wish to possess the mother and replace the father) are not demonstrated by the facts of the case.

2) Never having expressed either fear or hatred of his father, Hans was told by Freud that he possessed these emotions. On subsequent occasions Hans denied the existence of these feelings when questioned by his father. Eventually, he said "Yes" to a statement of this kind by his father. This simple affirmative obtained after considerable pressure on the part of the father and Freud is accepted as the true state of affairs and all Hans's denials are ignored. The "symptomatic act" of knocking over the toy horse is taken as further evidence of Hans's aggression towards his father. There are three assumptions underlying this "interpreted fact"—first, that the horse represents Hans's father; second that the knocking over of the horse is not accidental; and third, that this act indicates a wish for the removal of whatever the horse symbolized.

Hans consistently denied the relationship between the horse and his father. He was, he said, afraid of horses. The mysterious black around the horses' mouths and the things on their eyes were later discovered by the father to be the horses' muzzles and blinkers. This discovery undermines the suggestion

(made by Freud) that they were transposed moustaches and eye-glasses. There is no other evidence that the horses represented Hans's father. The assumption that the knocking over of the toy horse was meaningful in that it was prompted by an unconscious motive is, like most similar examples, a moot point. Freud himself (3) does not state that *all* errors are provoked by unconscious motives and in this sense "deliberate." This is understandable for it is easy to compile numerous instances of errors which can be accounted for in other, simpler terms[21] without recourse to unconscious motivation or indeed motivation of any kind. Despite an examination of the literature we are unable to find a categorical statement regarding the frequency of "deliberate errors." Furthermore, we do not know how to recognize them when they do occur. In the absence of positive criteria the decision that Hans's knocking over of the toy horse was a "deliberate error" is arbitrary.

As there is nothing to sustain the first two assumptions made by Freud in interpreting this "symptomatic act," the third assumption (that this act indicated a wish for his father's death) is untenable; and it must be reiterated that there is no independent evidence that the boy feared or hated his father.

3) Freud's third claim is that Hans's sexual excitement and desire for his mother were transformed into anxiety. This claim is based on the assertion that "theoretical considerations require that what is today the object of a phobia must at one time in the past have been the source of a high degree of pleasure" (p. 201). Certainly such a transformation is not displayed by the facts presented. As stated above, there is no evidence that Hans sexually desired his mother. There is also no evidence of any change in his attitude to her before the onset of the phobia. Even though there is some evidence that horses were to some extent previously a source of pleasure, in general the view that phobic objects must have been the source of former pleasures is amply contradicted by experimental evidence. Apart from the numerous experiments on phobias in animals which disprove this contention, (4, 10, 15) the demonstrations of Watson and Rayner (12) and Jones (9) have clearly shown how phobias may be induced in children by a simple conditioning process. The rat and rabbit used as the conditioned stimuli in these demonstrations can hardly be regarded as sources of "a high degree of pleasure," and the same applies to the generalized stimulus of cotton wool.

4) The assertion that Hans's horse phobia symbolized a fear of his father has already been criticized. The assumed relationship between the father and the horse is unsupported and appears to have arisen as a result of the father's strange failure to believe that by the "black around their mouths" Hans meant the horses' muzzles.

5) The fifth claim is that the purpose of Hans's phobia was to keep him near his mother. Aside from the questionable view that neurotic disturbances occur for a purpose, this interpretation fails to account for the fact that Hans experienced anxiety even when he was out walking *with his mother*.

6) Finally, we are told that the phobia disappeared as a result of Hans's resolution of his oedipal conflicts. As we have attempted to show, there is no adequate evidence that Hans had an oedipus complex. In addition, the claim that this assumed complex was resolved is based on a single conversation between Hans and his father (see above). This conversation is a blatant example of what Freud himself refers to as Hans having to "be told many things he could not say himself, that he had to be presented with thoughts which he had so far *shown* no signs of possessing, and that his attention had to be turned in the direction that his father was expecting something to come" (p. 246).

There is also no satisfactory evidence

[21] See for example the experiments on learning and habit interference (11, 16).

that the "insights" that were incessantly brought to the boy's attention had any therapeutic value. Reference to the facts of the case shows only occasional coincidences between interpretations and changes in the child's phobic reactions. For example, "a quiet period" early followed the father's statement that the fear of horses was a "piece of nonsense" and that Hans really wanted to be taken into his mother's bed. But soon afterwards, when Hans became ill, the phobia was worse than ever. Later, having had many talks without effect, the father notes that on March 13 Hans, after agreeing that he still *wanted* to play with his widdler, was "much less afraid of horses." On March 15, however, he was frightened of horses, after the information that females have no widdlers (though he had previously been told the opposite by his mother). Freud asserts that Hans resisted this piece of enlightenment because it aroused castration fears, and therefore no therapeutic success was to be observed. The "first real improvement" of April 2 is attributed to the "moustache enlightenment" of March 30 (later proved erroneous), the boy having been told that he was "afraid of his father precisely because he was so fond of his mother." On April 7, though Hans was constantly improving, Freud commented that the situation was "decidedly obscure" and that "the analysis was making little progress."[22]

Such sparse and tenuous data do not begin to justify the attribution of Hans's recovery to the bringing to consciousness of various unacceptable unconscious repressed wishes. In fact, Freud bases his conclusions entirely on deductions from his theory. Hans's latter improvement appears to have been smooth and gradual and unaffected by the interpretations. In general, Freud infers relationships in a scientifically inadmissible manner: if the enlightenments or interpretations given to Hans are followed by behavioral improvements, then they are automatically accepted as valid. If they are not followed by improvement we are told the patient has not accepted them, and not that they are invalid. Discussing the failure of these early enlightenments, Freud says that in any event therapeutic success is not the primary aim of the analysis,[23] thus sidetracking the issue; and he is not deflected from claiming an improvement to be due to an interpretation even when the latter is erroneous, *e.g.*, the moustache interpretation.

No systematic follow-up of the case is provided. However, fourteen years after the completion of the analysis, Freud interviewed Hans, who "declared that he was perfectly well and suffered from no troubles or inhibitions" (!). He also said that he had successfully undergone the ordeal of his parents' divorce. Hans reported that he could not remember anything about his childhood phobia. Freud remarks that this is "particularly remarkable." The analysis itself "had been overtaken by amnesia!"

AN ALTERNATIVE VIEW OF HANS'S PHOBIA

In case it should be argued that, unsatisfactory as it is, Freud's explanation is the only available one, we shall show how Hans's phobia can be understood in terms of learning theory, in the theoretical framework provided by Wolpe (14). This approach is largely Hullian in character and the clinical applications are based on experimental findings.

In brief, phobias are regarded as conditioned anxiety (fear) reactions. Any "neutral" stimulus, simple or complex, that happens to make an impact on an individual at about the time that a fear reaction is evoked acquires the ability to evoke fear subsequently. If the fear at the original conditioning situation is of high intensity or if the conditioning is many times repeated

[22] By Freud's admission Hans was improving despite the absence of progress in the analysis.

[23] But elsewhere (p. 246) he says that a psychoanalysis is a therapeutic measure and not a scientific investigation!

the conditioned fear will show the persistence that is characteristic of *neurotic* fear; and there will be generalization of fear reactions to stimuli resembling the conditioned stimulus.

Hans, we are told, was a sensitive child who "was never unmoved if someone wept in his presence" and long before the phobia developed became "uneasy on seeing the horses in the merry-go-round being beaten" (p. 254). It is our contention that the incident to which Freud refers as merely the exciting cause of Hans's phobia was in fact the cause of the entire disorder. Hans actually says, "No. I only got it [the phobia] then. When the horse in the bus fell down, it gave me such a fright, really! That was when I got the nonsense" (p. 192). The father says, "All of this was confirmed by my wife, as well as the fact that the anxiety broke out immediately afterwards" (p. 193). The evidence obtained in studies on experimental neuroses in animals (*e.g.* 14) and the studies by Watson and Rayner (12), Jones (9) and Woodward (15) on phobias in children indicate that it is quite possible for one experience to induce a phobia.

In addition, the father was able to report two other unpleasant incidents which Hans had experienced with horses prior to the onset of the phobia. It is likely that these experiences had sensitized Hans to horses or, in other words, he had already been partially conditioned to fear horses. These incidents both occurred at Gmunden. The first was the warning given by the father of Hans's friend to avoid the horse lest it bite, and the second when another of Hans's friends injured himself (and bled) while they were playing horses.

Just as the little boy Albert (in Watson's classic demonstration, 12) reacted with anxiety not only to the original conditioned stimulus, the white rat, but to other similar stimuli such as furry objects, cotton wool and so on; Hans reacted anxiously to horses, horse-drawn buses, vans and features of horses, such as their blinkers and muzzles. In fact he showed fear of a wide range of generalized stimuli. The accident which provoked the phobia involved two horses drawing a bus and Hans stated that he was more afraid of large carts, vans or buses than small carts. As one would expect, the less close a phobic stimulus was to that of the original incident the less disturbing Hans found it. Furthermore, the last aspect of the phobia to disappear was Hans's fear of large vans or buses. There is ample experimental evidence that when responses to generalized stimuli undergo extinction, responses to other stimuli in the continuum are the less diminished the more closely they resemble the original conditional stimulus.

Hans's recovery from the phobia may be explained on conditioning principles in a number of possible ways, but the actual mechanism that operated cannot be identified, since the child's father was not concerned with the kind of information that would be of interest to us. It is well known that especially in children many phobias decline and disappear over a few weeks or months. The reason for this appears to be that in the ordinary course of life generalized phobic stimuli may evoke anxiety responses weak enough to be inhibited by other emotional responses simultaneously aroused in the individual. Perhaps this process was the true source of Little Hans's recovery. The interpretations may have been irrelevant, or may even have retarded recovery by adding new threats and new fears to those already present. But since Hans does not seem to have been greatly upset by the interpretations, it is perhaps more likely that the therapy was actively helpful, for phobic stimuli were again and again presented to the child in a variety of emotional contexts that may have inhibited the anxiety and in consequence diminished its habit strength. The *gradualness* of Hans's recovery is consonant with an explanation of this kind (14).

CONCLUSIONS

The chief conclusion to be derived from our survey of the case of Little Hans is that it does not provide anything resembling

direct proof of psychoanalytic theorems. We have combed Freud's account for evidence that would be acceptable in the court of science, and have found none. In attempting to give a balanced summary of the case we have excluded a vast number of interpretations but have tried not to omit any material facts. Such facts, and they alone, could have supported Freud's theories. For example, if it had been observed after Gmunden that Hans had become fearful of his father, and that upon the development of the horse phobia the fear of the father had disappeared, this could reasonably have been regarded as presumptive of a displacement of fear from father to horse. This is quite different from observing a horse phobia and then asserting that it must be a displaced father-fear without ever having obtained any direct evidence of the latter; for then that which needs to be demonstrated is presupposed. To say that the father-fear was repressed is equally no substitute for evidence of it.

Freud fully believed that he had obtained in Little Hans a direct confirmation of his theories, for he speaks towards the end of "the infantile complexes that were revealed behind Hans's phobia" (p. 287). It seems clear that although he wanted to be scientific Freud was surprisingly naive regarding the requirements of scientific evidence. Infantile complexes were not *revealed* (demonstrated) behind Hans's phobia: they were merely hypothesized.

It is remarkable that countless psychoanalysts have paid homage to the case of Little Hans, without being offended by its glaring inadequacies. We shall not here attempt to explain this, except to point to one probable major influence—a tacit belief among analysts that Freud possessed a kind of unerring insight that absolved him from the obligation to obey rules applicable to ordinary men. For example, Glover (6), speaking of other analysts who arrogate to themselves the right Freud claimed to subject his material to "a touch of revision," says, "No doubt when someone of Freud's calibre appears in our midst he will be freely accorded . . . this privilege." To accord such a privilege to anyone is to violate the spirit of science.

It may of course be argued that some of the conclusions of Little Hans are no longer held and that there is now other evidence for other of the conclusions; but there is no evidence that in general psychoanalytic conclusions are based on any better logic than that used by Freud in respect of Little Hans. Certainly no analyst has ever pointed to the failings of this account or disowned its reasoning, and it has continued to be regarded as one of the foundation stones on which psychoanalytic theory was built.

SUMMARY

The main facts of the case of Little Hans are presented and it is shown that Freud's claim of "a more direct and less roundabout proof" of certain of his theories is not justified by the evidence presented. No confirmation by direct observation is obtained for any psychoanalytic theorem, though psychoanalysts have believed the contrary for 50 years. The demonstrations claimed are really interpretations that are treated as facts. This is a common practice and should be checked, for it has been a great encumbrance to the development of a science of psychiatry.

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THE CROSS-DRESSER: A CASE STUDY

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Recent reviews of the literature of transvestism have indicated the very unsettled state of psychodynamic theory in this region of sexual behavior. It is probable that an important factor in the diversity of views is the character of the available data. While in actual amount it is fairly copious, its quality often seems questionable. In the writer's experience with sex deviates, the verbal ability and insight capacity of the subjects were limiting factors even when intelligence and accessibility were not.

The subject of this report is not only of far above-average intelligence but has exceptional facility in verbalizing his feelings and in making discriminations; likewise marked ability for self observation and analysis. A university student in physical science, his motivation—in part, self-understanding and hope of "cure"—seems a fair guarantee of the reliability of the material.

Apart from these features the history offers an unusually analytic account of shoe fetishism and some significant observations on the concept of the "phallic woman," as well as data bearing on the widely current psychoanalytic interpretation of transvestism.

In view of the extreme importance of the feminine shoe in the subject's sexuality, his classification as a transvestite rather than as a fetishist may seem questionable. While he has rarely dressed fully in woman's clothing, his fantasy productions indicate marked preoccupation with all the details of feminine apparel and cosmetic, and with the experience of "feeling feminine" in dress, in manner and in speech, and in being treated and accepted as feminine. It is, moreover, the *wearing* of feminine shoes that is the primary erotic stimulus. He appears as a transvestite with a fetishistic interest in shoes.

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THE HISTORY

The subject is an only child, raised principally by his mother, grandmother, and several women who held the positions of cook and nursemaid during his childhood and youth. He saw little of his father, owing to parental divorce during early childhood. He is now 29, a man of medium height and build who describes himself as "definitely masculine" in appearance.

In a diary kept by his mother he once found a photograph of himself, taken at the age of about three, in which he was dressed in girl's clothing and was wearing a hair ribbon. He was unable to learn how he happened to be dressed in this way.

"My mother did tell me that when I was two years old I used to wear the maid's shoes, but I do not remember this either. I do recall that when I was four I had a very attractive girl of 17 for a nursemaid, and whenever she wore high-heeled shoes I would pester her until she let me put them on. One pair in particular fascinated me: they were brown and white spectator pumps, and I used to wear them for hours when I could get her to permit it. I still have an especial interest in this type of shoe, but age has made me more tolerant in accepting any shoe as a fetish object, provided it is high-heeled and distinctly feminine.

"Although I cannot remember precisely the feelings that went with wearing high-heeled shoes at the age of four, I do know they felt soft and unusual, and the wearing was accompanied by a distinct sense of achievement. In particular I recall that this nursemaid once called me over to her to see something or other, and despite the fact of their clopping sound, she made no com-

ment about my wearing high heels. This gave me a great thrill, I recall, and even now I longingly think of that moment and want to re-live it.

"As I grew older I began to be more and more interested in women's shoes. The family next door to us had two daughters, and I found I could wear the shoes of one of them quite well. I will never forget the shock and thrill when I sneaked up to her bedroom the first time, tried on her black suede pumps, and found that they fit me. The feeling—or rather the memory of it—is more clear this time: it was a sense of enclosure of my foot in the soft material, of a sense of imbalance on the high heels, and of a vivid knowledge of wrongdoing, accompanied by a simultaneous fear and hope of being caught.

"Later on, I had fantasies about the idea of putting on high-heeled shoes with laces or straps, so that if anyone approached I would be unable to get them off without being caught. In the light of this and later experiences, I believe I can satisfy your curiosity about one reason, at least, for offering my case history: it is a way of "being caught" that satisfies this component of my desire, while avoiding the drastic denouement that would follow a revelation to anyone but a psychologist or psychiatrist.

"I began masturbating at age 13, the first episode taking place while gazing at an advertisement for high-heeled shoes in a fashion magazine. Such magazines, together with Sunday newspapers, furnished me with excitement and masturbatory material for several years, during which my episodes of wearing women's shoes were at a minimum.

"My mother died when I was 12, and I began to expand my activities. I bought a pair of black suede pumps at a rummage sale, and wore them when I was home from boarding school on vacations. While I was away at school I bought semi-pornographic "girlie" magazines, showing women in 5″ to 7″ high heels, and masturbated while having fantasies of either wearing the shoes of my own volition or of being forced to wear them by women."

The subject reports himself as a fair enough "mixer" and as fully accepted socially. He was graduated with high honors and was accepted at a well-rated mid-western university.

He reports as follows an experience of the summer before entering college. He had decided to buy a pair of high-heeled pumps. Buying the shoes was an ordeal in itself; he describes himself as breathing heavily, blushing, feeling weak in the knees, yet thrilled at what he felt was the forbidden character of the deed.

"I got out of (the store) somehow and drove as if half-drunk until I found a deserted spot and pulled off the road. With my hands shaking and sweaty I took the shoes out of the box, pulled off my own, and tried to put them on. They were too small! Frantically I pushed and tried to jam my feet into them, but to no avail. At last, shaking now with rage, I drove to the next town and bought a pair of nylon stockings, hoping they would be thin enough that I could get the shoes over them.

"Again back at my deserted spot I got out the nylons. I tried to put them on simply by putting my foot in at the top, but soon found I had to roll them down first. As I did so I looked up fearfully from time to time to see whether anyone was approaching. At last I got the stockings on my feet, and, with my penis erect and throbbing under my pants, I managed to get the high-heeled shoes on my feet. They were terribly tight, but in spite of that I got out of the car and took a few timid steps. It was a new thrill to wear high heels out of doors, and it so magnified my excitement and fear of being caught that by the time I had walked half way around the car I could contain myself no longer. I took out my penis and masturbated by the roadside.

"No sooner had I ejaculated than a wave of disgust swept over me. Where they had

been tolerable before, now the tight shoes were excruciating, and in disgust I pulled them off and flung them into the underbrush along with the stockings. I got back into the car and drove away from the shameful scene. The disgust soon left, however, and it was not long before I bought another pair of shoes, these in size 10-C, both longer and wider than the others. These, a pair of black suede sandals with three buckles, were ideal for my getting-caught idea, for it would be impossible for me to take them off quickly. To further magnify the possibility of getting caught I once walked several yards from my car in broad daylight, but when a car approached I retreated in fear.

"I have described this action in detail because it is characteristic of my actions in such an episode. I first get excited by seeing high-heeled shoes in pictures, on women, or in my imagination, the scene being accompanied by a strong erection and many fantasies. At the first opportunity I drive to a nearby city and go to a store that sells women's shoes. Since the clerks are usually men, I make light of the fact that I am buying shoes for "her" (I seldom find it necessary to specify the "her"), and complain about the difficulty of finding high heels in size 10-C. Incidentally, during these times I have an erection constantly, and the speaking of the forbidden words "high heels" or "women's shoes" nearly chokes me.

"When I have bought the shoes I hurry to my car to try them on; I wear nylons under my socks at such times. From the moment I open the shoe box and start to put the shoes on my erection is extreme. I try to wear them as long as possible without allowing myself to masturbate, and this length of time varies with my mood. There was a period at college during which my passion for women's clothes reached its peak. I began by buying a pair of nylon panties, then a garter belt, then a girdle, and, still later, slips, petticoats, skirts and blouses. I would rent a hotel room and install myself, together with these clothes and a collection of high-heeled shoes. First of all I would put on the underclothes and silk stockings, then the blouse, and over these I would wear my own pants and overcoat, buttoned to the chin. I had a pair of overshoes which I would put on over a pair of medium-heeled shoes, and go out on the street thus clad. I once walked several blocks in this costume in the downtown section of a city. I am amazed that no one noticed my restricted gait and frightened look to the extent of calling a policeman. I had just enough good sense not to wear extreme high heels, for these would surely have had this result."

On one occasion the subject, dressed completely as a woman, took a short walk in public, but was thrown into a mild panic by a male passer-by who stopped him to inquire directions. He did not venture to do this again. On another occasion he left his car while wearing high-heeled pumps and walked some distance down a residential street.

"The sound of a motor brought me to my senses, and in a panic I realized I was standing on the street in high-heeled shoes. I started back to my car at as fast a walk as I could manage, but I was not half-way there when I realized the school bus would pass me before I could gain the safety of my car. The words came to my mind, "They'll catch me in high heels," and at that moment I knew I was about to ejaculate. The orgasm was so powerful that I nearly doubled over, and it lasted several seconds, during which I managed to get to my car just as the bus passed. The sense of revulsion I expected did not come at once, but when I got to a gas station rest room and attempted to clean up the mess I was indeed disgusted.

"Since then I have attempted several times to reënact the scene. I now park on a residential street, preferably on a cloudy or rainy day (to keep people indoors) and

get out of my car with high heels on. The thrill I get now out of putting on a new pair of high-heeled shoes is not nearly so great as that of walking in public in them. Further, on the occasions I have been successful in achieving orgasm without masturbation, it has always been on my way *back* to my parked car, when I know that the nearest hiding place is still many steps away."

The subject reports his urge to wear feminine shoes as at times uncontrollable in intensity. He describes a phase of one episode as follows. While studying, he thought "of how it would be to put on high heels and walk in public, in the way I have done so often. All at once I got the feeling of riding on a plunging elevator; my testicles began to rise, my abdomen felt as though it was turning to jelly, my arms were limp, and the glands at the back of my jaw seemed to swell and grow warm. I sat still for a few moments to try to record all the sensations in my mind, and then got up to go to my car where I had the shoes hidden." His teeth chattering from "anxiety and anticipation," he then sought a place where, without too great a risk he could leave the car for a few seconds while wearing the shoes. He might for example, do no more than mail a letter, or put a coin in a parking meter. Depending partly on organic condition, he might achieve orgasm at such a time. Again, if the risk were too great, anxiety might cancel his excitement. Once, after finding some shoes in a garbage can, he took them to his car in great excitement. "When I can see that they are a large size and they might fit me, I'm breathing so hard when I drive off that I sometimes almost groan; I can hardly drive, I get so excited. If I try them on and they fit, its even *more* exciting. My hands shake like hell . . . and my knees are shaking. If I decide to drive to a spot where I can let people see me in them for a moment . . . my foot shakes so much on the gas pedal that the car jumps and bucks, and I can't stop it shaking no matter how hard I try."

The subject married at the age of 23. At first his wife accepted his fetishism to the point of allowing him to wear his favorite shoes during intercourse. When later she refused to continue this practice, marital relations grew less frequent and masturbation, with fetish stimulation, more frequent.

He gives a number of examples of his ventures in risking detection of his urge to seek "thrills from exposure." He visited a shoe store, saying that he needed high-heeled shoes in order to take the part of a girl in a play. "I put out my foot, and as the clerk slipped the high-heeled shoe on my foot I broke out in a sweat and started shaking. I put on the other shoe myself, but when I started to stand up I saw that my erect penis was bulging my pants. I managed to correct the difficulty and took a few steps on the high heels, over to a floor mirror where I could observe them on my feet." When the onlookers became increasingly suspicious on noting his nylon stockings, he fled from the store, "a nervous wreck, shaking, sweating, and gasping." But the experience had been "too thrilling to ignore" and he repeated it. If a first visit to a store failed to attract attention he would come a second time, wearing increasingly more conspicuous feminine footwear until he obtained the desired reaction.

THE SHOE AS FETISH

While the core of the subject's transvestism is the shoe, this fetish is only to a small degree a direct sexual stimulus; it becomes sexually arousing primarily when worn, in reality or in imagination.

"Let me put it this way: when I wear high heels with men's clothing I become highly aroused, masturbate, and that is that. But when I wear women's clothes, such as stockings, slip, etc., I still hunt about, almost frantically, for high heels to complete the costume, and, even though I may be aroused to the point of masturbation, it is unsatisfying without high-heeled shoes. My "Great Impersonation" [a brief venture in

complete feminine attire] was primarily to enable me to wear high heels in public with at least the illusion of acceptability.

"Further, I have sometimes masturbated by pulling a stocking over my penis and putting my organ into a high-heeled shoe, rubbing it up and down until orgasm took place. Once . . . I wore a shoe on my penis in the street, covering it with my buttoned overcoat. I was tempted to exhibit it, but good sense prevailed. All this, it seems to me, indicates fetishism, in the importance I attach to high-heeled shoes. On the transvestite side, I had a great thrill—as, I suppose, did all transvestites—at the Christine Jorgenson transformation (also Roberta Cowells and Charlotte MacLeod, similar cases); this seems to be the ultimate goal of the pure transvestite: to become, ostensibly, a woman. I do not share in this wish, but prefer to remain a man in female clothes." Elsewhere the subject writes, "high heel *fantasies* are a necessary condition for masturbation and also for sex relations with my wife . . . no high-heeled shoes (imagined or real), no sex. . . . I have *never* been in a state of sexual excitement except through fetishism."

A shoe may be feminine or it may be "phallic." By phallic the subject means "erotically bisexual." While for people in general high heels are essentially feminine, for the subject "they represent something aggressive and hence phallic." He becomes speculative concerning what he means by an aggressive quality in this context. He associates high heels with masculinity, for they make a person taller, "and their firm clicking on the pavement would give a man a signal of his self-assertiveness. His appearance, both visually and audibly, would convey a sense of authority. In this way I satisfy an authoritarian and phallic urge when I put on high heels."

But high heels are ambivalent because they are also feminine. ". . . they are quite basically related to the Chinese bound-foot concept, both in appearance and intent: they form a curvaceous (hence feminine) instep, and they hobble the woman and make her subservient, or at least inferior in ambulatory potential, to the man. This is borne out by a statement I saw in an advertisement for high heel movies: 'the restricted gait of our models in high heels will thrill and delight you'. This, then, represents an aspect of masochism which I satisfy by putting on high heels: I 'bind' myself, in a sense, and make myself feminine. . . . Both aspects are borne out by my preference for what I described as phallic shoes: leather ones, with buckles (another symbol of authority) and extreme heels: they restrict masochistically and they give authority phallically. Feminine (pink satin) shoes with high heels, on the other hand, possess a strong bias on the masochistic side, since their appearance is exclusively feminine and the authoritarian aspect is minimized."

The subject makes a detailed analysis of his reactions to the feminine shoes (about 60 pairs) he has owned. His vocabulary shows marked technical familiarity with styles and structures of women's shoes. It is also clear that each shoe has had for him a certain individuality. He recalls in detail the shoes he preferred at different ages back over the years, and noting the exact heel-heights.

The erotic value of heels increases rapidly with height beyond the moderate degrees. For flat heels desire is small, "but as the heel rises to, say, 2″, my desire increases a little bit with each inch. From 2″ to 4″ my desire increases more rapidly, as much per each *half* inch height as it did per *one* inch in the lowest range. Then from 4″ to 5″ it increases that same amount for every ¼″ of heel height, and beyond 5″ it is at its maximum. Toward the end, say from 6½″ on up, it begins to decrease, since the heels look so exaggerated that they are ridiculous."

Exemplifying "phallic" shoes are "all calf shoes, all boots, all spectator pumps, all dark suede shoes with buckles, all shoes

with complex strap arrangements." Among "feminine" shoes are "all pastel-colored shoes, all satin shoes, all shoes with fluffy, frilly decorations."

The subject describes the erotic values of all varieties of shoes by way of a matrix arrangement. In the columns are shoes of the same style, with varying heights of heel; in the rows are shoes of the same height of heel, but of different styles. The continuum from "phallic" to feminine extends from left to right. The highest heels are at the top, the lowest at the bottom. At the vertical midline of the matrix he puts "all unadorned black, dark blue, and dark brown suede shoes." The black ones he puts on the line, the brown a little to the left (phallic) side, and the blue a little to the right.

He illustrates with a row: "all the shoes have, let us say, 4″ heels. Starting at the dividing line . . . my desire increases at the same rate on both sides, phallic and feminine: just a little at first, when all the shoes are still rather conservative pumps, and increasing more and more rapidly toward the extrêmes of fur-trimmed carriage boots and pink satin pumps."

The heels of his favorite shoes are a potent stimulus. He may break the heels off shoes he finds if they are too small to wear. "I play with the heels and rub them against my penis, and put them in my mouth and suck them (phallic, no?). Once I whittled the sharp edges off a high heel and rounded the bottom and stuck it in my rectum. It made me very sexy—."

HOMOSEXUALITY

"While I have never experienced any degree of sexual attraction to any man who was in my presence, I have had lengthy and elaborate fantasies concerning relationships with imaginary men." The subject states he experienced no attraction to any of three homosexuals he has met, even when wearing fetish shoes. "I have, however, a strong attraction to pictures of female impersonators, having masturbated frequently while looking at such pictures. . . . The thing I least like to admit is that I have often fantasied relations with a man: I imagine a man of very feminine build, dressed in women's clothes, and I imagine myself lifting his skirts and performing fellatio on him. Sometimes I think of men, dressed as men, forcing me to wear women's clothes and to perform fellatio on one while another performed rectal intercourse on me."

With but a single exception, the subject states, his reaction to fantasied men "exists solely when I am in a state of excessive fetishistic excitement. . . . 'sex' and 'perversion' have meant one and the same thing to me. Any perverted fantasy I have had, whether masochistic (*never* sadistic—I *hate* the thought of hurting anyone or any animal) or homosexual, has come about in an effort to excite myself beyond the stage I happen to be in at the time, which state, in turn, is the result of thinking about wearing high-heel shoes." The subject's homosexual inclinations are exclusively sensual. "Romantic" and "homosexual" are for him antithetic concepts, the one meaning love without sex, the other sex without love. . . . "As far as thinking of myself as a woman in the 'full' (*i.e.* menstruating, childbearing, penis-less) sense, I have tried, but I dislike it. I have no inclination to be such a person."

The subject lists his transvestite fantasies in order of increasing homosexuality. 1) An older woman allows him to put on her clothing; he dwells on the details; the fantasy concludes when the woman masturbates him. 2) A group of women force him to put on very feminine clothing, "satin sheath dresses or fluffy, billowy night gowns," while the women themselves dress in "phallic style": "tailored suits, calf pumps, very business-like." They put stockings on his penis, a high-heeled shoe over the stocking; he walks about in high heels until he has an orgasm. Sometimes the women are "highly phallic"; they force him to wear extremely high heels and they bind him into a corset.

They expose his penis, make him lick their shoes, force their shoes on his penis, tease him until he ejaculates. 3) He fantasies having the body and features of a "very lovely" woman, though retaining his penis. He revels in thoughts of "soft and voluptuous clothing." 4) Dressed as a woman, he has overt homosexual relations with another man, also dressed as a woman; again, dressed in high heels and corsets, bent over a table and bound, he is subjected to homosexual acts by men.

HETEROSEXUAL EXPERIENCE

The subject's first heterosexual attraction occurred at age 16, to a girl of 18. This brief relationship, which involved considerable "necking," he reports as "exciting" but altogether free of sensual interest; the emotion was romantic in quality. Soon after this he "fell in love" briefly with a girl of 14. Other experiences followed, involving more "necking" but without intercourse, though one episode included sensual maneuvers, erection, and a desire for coitus. During this period the subject was engaging in occasional transvestite ventures. Age was about 18.

At 23 he met the woman, 8 years older, who became his wife. This relationship was a love experience, but included sensual attraction. The first intercourse was enjoyable but "sensually no better than masturbation," though made exciting by its "sinfulness" because they were not yet married. Having demonstrated his masculinity, his thoughts soon reverted to transvestism; chapters in the courtship of his wife were interspersed with episodes involving high heels. Marital relations were most satisfying when his wife permitted him to wear high-heeled shoes and nylons; however, she was often resistive to this practice. "She evolved the cagey scheme of limiting my spending money so that I can never scrape together enough to buy a pair of shoes, but by touring garbage routes and prowling through Salvation Army stores I manage to get a pair now and then. I never keep them long, because I fear she will find them; I wait until a post-masturbatory disgust period and throw them away."

BACKGROUND FACTORS

The subject was presented with a series of questions concerning the psychological origins of his transvestism; *i.e.* parental rejection because of sex; dressing, as a child, in the clothing of the opposite sex; identification with the parent of opposite sex, etc.

In response he states that he was not rejected by either parent; that, in fact, his parents were over-solicitous, and he relates the excessive attention he received to the fact that he was a "full-fledged sissy" for the first ten years. "Everyone said so," and he recalls the "sing-song chorus" of childrens' voices that plagued him wherever he went, with the hated epithet: "sissy." He recalls no incident of being forced to wear girls' clothing, and can cite only the photograph of himself in feminine apparel, at age three, noted above, plus the recollection that he once begged his mother to call him by a girl's name. He recalls no desire to be rid of his penis, nor any notable interest in it until he began to masturbate at age 13. As to parental roles, his father, he states, was emphatically the dominant member of the family, "lord and master of everything and everyone." He concedes, however, that his upbringing was engineered "almost exclusively by females." In this connection he stresses feminine clothing as a direct sensory stimulant. "I was 'babied' (over-protected), and as such I was quite aware of any bodily discomfort I felt, particularly from scratchy clothing such as tweeds. Further, being always with women, I had the opportunity to feel their clothing, finding it soft and smooth. Femininity and luxurious comfort became strongly associated in my mind, the original comfort-urge became a pleasure-urge, and I became transvestitic. Still further, comments like, "But only *girls* wear silk—boys *have* to wear scratchy

clothes'—substantiated the conclusion that being a boy was not a bed of roses."

The subject continues with an hypothesis of his own: "With appropriate fanfare, I hereby coin a term to describe myself: eclectic sexuality. Eclectic, because I pick from manhood the pleasurable attribute of a penis, the freedom from menstrual difficulties and childbirth, and the opportunity to be independent of others (not having to rely on a husband, for instance), while from womanhood I choose the ability to wear soft and voluptuous clothes, to admire myself and make a great fuss over my appearance, and to qualify for special social consideration. The result, of course, is my ideal: the phallic woman, free to do as she pleases, untroubled by periodic bleeding, possessing an obvious and tangible pleasure-organ, wearing smooth garments, shamelessly vain, and invariably catered to."

He disagrees strongly with the statement by Lukianowicz that transvestism is limited to cross-dressing, that there is no desire for a change of sex, while transsexualism is progressive, making steadily increasing demands. Our subject feels that in his case the difference is one of degree only. He notes his transition from merely wearing high-heeled shoes and walking in them, to the impulse to risk exposure while wearing them. His fantasies, as earlier indicated, have moved in a homosexual direction. He states that "an imaginative person . . . feels jaded after many repetitions of the same thing. The element of experimentation and the thrill of new sensations is gone after a number of trials, and he must seek a new way to gain pleasure. This is my case: five years ago I was violently anti-homosexual; now I dream of it. Twelve years ago I first put on nylon stockings, and I disliked it—now I wear them at every possible chance. Right now I have no desire for penotomy—ten years from now I may beg for the operation; I don't know."

THE PHALLIC WOMAN

Among the subject's fantasies there appears a significant figure: the "phallic woman." She appears in different guises; phallicism is, moreover, a matter of degree. The phallic woman is mature, in her middle thirties. She may wear a tailored suit; she is the business, executive, or secretarial type. She wears high-heeled pumps. Sometimes she wears boots with 6 or 7 inch heels. She may wear a tight-fitting uniform with a wide leather belt. She is dominating, imperious, even threatening. She is "sexually and socially aggressive," authoritarian. She wears "quasi-masculine clothes and does a man's job." She forces him into bonds which render him helpless, following this with genital stimulation.

Such a woman would represent extreme phallicism. A more moderate example would be seen in a tailored suit, long gloves, furs, dark-seamed stockings, pumps with buckles or with ankle-straps. *Feminine* clothing, on the other hand, means, for example, "print dresses, fluffy hats, tinted stockings, brightly colored shoes . . . flat heels, medium heels."

In his fantasies, with female figures, there is a "complementary-phallic" relationship. If the woman is phallic, he is feminine, and vice versa. In homosexual fantasies, if he is being sexually forced by masculine men, he is feminine in role and dress, but "I am as phallic as my partner if he is wearing women's clothes, and the relationship is gentle and mutual." In fantasies with women, he states "I always stay within the limit of the function Ph_{me} plus Ph_{her} equals constant, and the constant is a measure of the level of my interest in transvestism at the moment.

He writes further: ". . . I do not want to be a *real* woman having relations with men, but a *phallic* woman having relations with either real women or other phallic women. Further, when I imagine myself as such a creature, a woman equipped with a penis,

it is always in reference to clothes: my status would be that of the transvestite's idea of a woman, as a being with a penis who is not frowned upon for wearing exotic garments."

The subject's marriage is further illustrative: his wife is a secretary and a "tyrant." Her age was a strong positive factor in his choice, he states, and acknowledges that his "masochism" responds to her tyranny, that it "ties in with the bondage theme."

Concerning his "need to be caught" the subject cites this motive as a factor in his willingness to submit his case history; despite the assurance that his identity would be safeguarded he experienced his confession as a mode of being caught, and offers as a further interpretation of its meaning that it may symbolize a kind of Rubicon-crossing; that is, exposure would permanently stigmatize him and thus release him from the demands of normality. "... once I am firmly and decisively caught, my normal life is at an end; I would have to resign myself to nothing but transvestite activity the rest of my life, for the scarlet letter would be burned into my forehead. And that is why I want to be caught, I'll bet! Being caught would be the perfect excuse to give myself over to sex motives only; my superego or conscience or whatever, will not let me do it of my own volition, but with justification like that, my conscience could no longer protest! I guess I want to be sort of forced into absolute transvestism by the social forces that catch me in the act: self-display is in itself exciting, and the excitement is enhanced by the possibility of being caught and sentenced *not* to a term in jail, but to a term in *women's clothes*, the sentence being not for 30 days or 10 years, but *life!*"

The subject is familiar with the psychoanalytic doctrine pertaining to fetishism and transvestism. He had been told by age seven, he thinks, that women had no penises, but did not see an unclothed female until age 15, when he observed a nude four-year-old. "I was not amazed nor aroused, but felt a little squeamish ... as if I were witnessing a surgical operation." At 18, at a side-show, he saw an adult woman undressed, for the first time. "I had a strong impulse to reach for my own organs to make sure they were still there, and the experience would have been far worse had not there been a sufficient number of wisecracks from the audience to keep the proceedings in a bantering tone. ... I dreamed several times after that that my penis had fallen off, and I was trying to put it back on before somebody noticed my predicament." He reports that he had quite recently dreamed that he "was wearing a pair of white boots with five-inch heels. A woman measured the heels, said they were too high, and cut off an inch. I was very sad, for the thrill of the high heels was gone."

DISCUSSION

A comprehensive survey of the literature of transvestism has recently been made by Lukianowicz (2). Several essential features of the present case are consistent with the findings of his study. Transvestism is often associated with fetishism, homosexuality, and masochism.

Much of the data supplied by this subject is consistent with the general concept of psychological bisexuality. His idea of "eclectic" sexuality seems little more than this; to wear feminine clothes, to be indulged, to exhibit himself with vanity, to enjoy himself narcissistically,—and to have a penis. According to mood, he assumes either masculine or feminine roles. When he feels "phallic" he wears high heeled shoes; in a more feminine role he wears other items of feminine clothing as well. In the presence of an imagined "phallic" woman or an aggressive homosexual he sees himself in feminine shoes. In relation to a feminine woman, imagined or real, he takes the masculine position in intercourse and

wears phallic shoes "to accentuate the authoritarian component." In relation to a passive homosexual he feels they should dress alike, that they would be in equilibrium with respect to masculine and feminine components.

Nagler, in an historical review of the literature and a case study of foot fetishism, concludes that, "what is common to the triad of homosexuality, fetishism, and transvestism . . . is not the fear of castration . . . but rather . . . the fear of the male social role in its entirety in the face of an overwhelming sense of inadequacy and a low self-esteem (1). He sees the fetishist as "an insecure, passive, dependent, and inadequate male . . ." He finds evidence of a need, *e.g.*, in masochistic fantasies, to be subservient, slavish, a "handmaid."

There is considerable data from this case study to support Nagler's thesis. In an elaborate fantasy production the subject projects himself into the role of an orphaned youth of 19 who has become the ward of a "phallic" and very indulgent woman. She sympathizes fully with his desire to dress himself in feminine clothing. Significant is the fact that this person is the nursemaid—now much older but still attractive—who allowed him, as a child, to wear her high-heeled shoes. A notable feature of the story is the emphatic expression of a regressive urge. The effeminate youth of the fantasy, who is addressed by his phallic consort as "baby" and "honey," is in revolt against growing up, wants to know why he cannot "be himself" instead; he sulks when reminded of his near-adult status; he weeps, bites his thumb, takes refuge at her breast, and is pacified only when assured that he need never grow up, need never be a man. At a later place he asks her "what it's like to be a man." She answers that one must be strong and hard, "walk alone," and fight. He replies that he wants none of these things and would therefore rather be a woman.

Compensatory behavior took the form of weight-lifting, at one time, in which he developed a very powerful physique. He married in part, he states, to establish his masculinity. He also grew a beard.

In the fantasy cited he is conspicuously docile and infantile, a pretty young "phallic" girl who submits willingly to her companion's sexual advances. In this connection we note his marriage to an aggressive, domineering and much older woman. While he expressed some resentment toward this domination, there was evidence of acceptance, too. A clear token was his offer to the writer to come and stay with him for a period, and to do menial housework tasks for the privilege of wearing feminine clothing about the house.

The fact that the highest pitch of erotic excitement is achieved only when the subject is about to be caught is a challenging datum for sex psychology (recalling other instances of a linkage between sex and anxiety), but the importance of the feeling of *wrongness* as an excitant is again suggestive of the regressive features of the case. It is as if excitability had become fixated at a phase of development when sex and wickedness are closely associated,—when adult disapproval can make any act a sin. Thus our subject, otherwise probably "emancipated" enough, still argues warmly that his wearing of feminine shoes *must* be wrong because it is socially disapproved.

Nagler's view that the behavior of the transvestite may be seen as a defensive turning away—out of inadequacy feeling—from the male role, is thus well supported by much of our data. Yet this data is equally consistent with a quite different assumption, namely, that there is an inclination, a *turning toward*, the feminine role. The positive aspect is as important as the negative. Our subject takes great and genuine pleasure in every item of feminine clothing. Not only does he want high heels, he wants to walk in them with the grace and poise of a woman. His wanting to be *accepted* in a feminine role is more than a de-

fense; it has the flavor (as he writes of it) of a deeply gratifying experience. His masochism is more than a renunciation of maleness; it is a fulfillment of femaleness. The psychoanalytic interpretation of transvestism as a defense from castration anxiety fails to fully consider this positive aspect of the experience itself. It may be a passion as well as a compulsion.

Significant, too, is the fact that the phallicism of the "phallic" woman includes much more than the penis so important to the Freudians. She is sharply authoritarian and masculine. Her phallicism means a certain hardness, imperious control, even brutality. Her unfeminine clothing is notable too; likewise the whip she sometimes carries, also "phallic." She is far more than the sexual symbol suggested by her name. She is distinctly a *personality type*.

In this context the meaning of the common fantasy-theme of *bondage* may be interpreted. Quantities of materials of this character are marketed by the publishers of semi-pornographic drawings and photographs. The subject made use of these materials and also reports a category of fantasies in which he is bound and subjected to sexual stimulation by phallic women or by men. Bondage offers direct gratification to the masochistic urge. It may also resolve the conflict generated by the assumption of the passive or feminine role by a male. The bondage symbol, verbalized, means: against my will I am forced to act like a woman and to abandon myself to masculine-aggressive caresses. Otherwise, whatever of the masculine attitude is present will be "in protest" against the act of sexual submission. The subject, in viewing these materials, projected himself into the role of the bound female.

There is no paucity of theory or of theoretical discussion in the contemporary literature of sex deviation. There might conceivably be less of both, and less need of both, if a greater amount of reliable data were available. Such data is admittedly not easy to obtain in a region where moral barriers must be surmounted in addition to the usual difficulties of exploring the patient's mind. The present status of our knowledge in this area of sex psychology indicates, in the writer's view, and with reference especially to psychoanalytic literature, that the impulse to interpret is too often more active than willingness to collect facts.

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INCONGRUOUS GENDER ROLE: NONGENITAL MANIFESTATIONS IN PREPUBERTAL BOYS[1]

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INTRODUCTION

Most reports of childhood incongruities of gender role and orientation[3] have been obtained from adult homosexuals and transvestites who reconstituted for investigators their childhood experiences. Furthermore, most studies dealing specifically with children concern cases obtained through police courts or reform schools and represent an extremely biased sample of this type of aberrant behavior. In part to overcome these limitations, and in part as an outgrowth of hermaphroditic studies done at this hospital, the present investigation was undertaken.

Money, Hampson and Hampson (9, 20, 21, 22) studied human hermaphrodites who had been designated one sex at birth and subsequently reassigned as a member of the other sex. They found that, when the reassignment was made after approximately two years of age, it could not be done without the risk of resultant abnormalities of gender orientation. A clean break with the originally assigned sex could not be made by children past this critical age. They explained this phenomenon using the concept of "imprinting" as first employed by Lorenz early in the 1930s (17).

Lorenz's classic experiment demonstrated that the mother-following reaction in newly hatched mallard ducklings can be manipulated experimentally to become imprinted to any substitute object that has the correct height-width ratio and moves. Lorenz squatted and substituted himself for the ducklings' mother. Thereafter the birds followed him, and only him around with the same devotion as they would otherwise their true mother. This imprinting of a perceptual stimulus is a kind of learning that will occur only during a limited critical period in the life cycle. Once having occurred, it is remarkably permanent and unmodifiable. Lorenz similarly imprinted newly hatched jackdaws and found that when these birds reached sexual maturity they imprinted not another jackdaw but a human being as sexual partner, courting either Lorenz or some other person.

PRESENT STUDY

Five cases, all boys, are reported in this paper. One boy has been followed for four years. The other four were referred during the past year in response to a request to practicing pediatricians and educators for young children who showed incongruities of gender role. Without this request, these children would almost certainly not have received any special attention for their gender-role problem. The signs warranting referral were specified as: excessive and persistent attempts to dress in the clothes of the opposite sex; constant display of gestures and mannerisms of the opposite sex; preference

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[3] By the term *gender role* is meant "all those things that a person says or does to disclose himself or herself as having the status of boy or man, girl or woman, respectively. It includes, but is not restricted to sexuality in the sense of eroticism" (22). Gender orientation is gender role as subjectively experienced.

for the play and other activities of the opposite sex; or a stated desire to be a member of the opposite sex.

A series of interviews was conducted with each child, his parents, and, in some instances with his siblings. In each case, representative interviews were recorded, and then transcribed. Physical examinations were given each child. During the course of the study approximately twenty hours were spent with each subject and his relatives for the initial workup. Followup visits were scheduled at six-month intervals. To illustrate the diversity of these cases, excerpts from each of the five cases are presented under each ensuing subheading. In addition, comparisons are made with the observations of other investigators.

PRESENTING COMPLAINTS

Boy One was first seen in 1955, at age seven. He had told his mother he was praying to God to change him into a girl. He had always played with dolls, which he easily wheedled his father into buying for him, and with other girlish playthings, by choice. He had always had an intense desire to dress in female clothing, accessories and high heels. *Boy Two* was eight when brought to the clinic in 1959 for the first time. The mother said, "He seems to wear my clothes whenever he has the opportunity. . . . That combined with the fact that some of his actions look effeminate and he plays only with girls in school." *Boy Three*, aged 10, was brought to the clinic in 1958 by his foster mother who had been caring for him a year and a half. She said he wiggled like a girl when dancing, and played mothers and fathers with his two foster brothers and was always the mother. The teacher said he seemed not to take up boys' activities at school, but would much rather be with a bunch of girls. *Boy Four* was five when brought to the clinic in 1959. He had an obsession for hanging out laundry, preferred playing with girls than boys and dressed up in his sister's clothing. *Boy Five* was brought to the clinic by his mother in 1959 when he was ten. "When he was younger, I'll say five, he used to wear my clothes—put on underwear, my dresses, earrings; he liked to paint up—powder and rouge and lipstick. . . . He doesn't want to play baseball, he's afraid of getting hurt. . . . His voice sounds like a girl."

There were no complaints of homosexual play or experience in these children. In fact, there were no complaints of sexual play or erotic experience of any type. Masturbation had not been an obtrusive problem. All the parents were aware of the existence of the phenomenon of homosexuality. They varied in the explicitness with which they had faced up to the conjecture that their son's present effeminacy might portend homosexual behavior in the future.

As a point of interest, it may be noted that the two parents, or foster parents, of their own decision accompanied each child for the initial visit. In all five instances, the husband viewed the problem as being less serious than did his wife. He was aware of the same symptoms in the boy's behavior as was his wife, but was more ready to minimize them as something the boy would outgrow. None of the fathers gave any indication of previous experience with such a sequence of behavioral development, either in himself or someone else.

AGE AT ONSET OF SYMPTOMS

Trite but true: the child is father of the man. But in observing an adolescent or adult with misplaced gender identification, it is easy to overlook the fact that he was once a child with the same problem. How far back did we trace our children's effeminate behavior?

Boy One: Pictures produced by the mother showed the boy at three dressed up to go out, carrying his favorite lady's pocketbook. At two he showed tremendous delight when dressed as a girl for Hallowe'en. *Boy Two:* The parents recognized retrospective signs after the first grade teacher made mention of effeminacy at age six. *Boy Three:* The

mother, hospitalized with schizophrenia, was unavailable for questioning. *Boy Four:* Symptoms were retrospectively dated to age four, at which time the mother was confined to bed after a stove explosion. *Boy Five:* A woman friend told the mother she had noticed effeminacy from babyhood. The mother recalled his dressing up at five.

STATED DESIRE TO BE OF THE OPPOSITE SEX

Boy One: was seven when he prayed God to change him into a girl. *Boy Two:* Which do you think it's better to be a boy or a girl? "A boy." Why? "Because that's how I was born." *Boy Three* answered: "Sometimes," to the question whether he ever wished he were born a girl. He was unable to furnish any reasons why. *Boy Four:* Do you mean that you wish you were a girl? Did you shake your head? Which is it best to be, a boy or a girl? "A boy." *Boy Five* "didn't know" if it made him feel good when he dressed up; he wished he were a girl "sometimes." However he would not go into a hospital or take medicine to turn into a girl, supposing that that were possible.

PARENTAL RELATIONSHIP

A good deal has been written about the mother-father relationship of effeminate boys, especially the submissive father. The following relevant observations were made of these children and their parents.

Boy One: There was smouldering, outspoken hatred between his parents, with bouts of open war. Themselves childless, through lack of proper copulation, they had adopted the patient. The husband abdicated family leadership willingly to his wife. *Boy Two:* The marriage relationship had all the earmarks of being a good one. The wife ran the household affairs. *Boy Three* came from a Negro household in which the father had been absent since the boy's infancy. His place was taken by a nomadic stepfather, but the maternal grandfather was really the significant male in the boy's life. When the grandfather died, the boy at the age of eight was placed in a foster home. *Boy Four:* These parents gave a sentimental, idealized picture of their relationship, but did, however, present a contented one. They had intercourse as infrequently as once a month. *Boy Five:* This was a perfunctory, mended marriage after a two-year separation due to the husband's drinking. The mother was the dominant partner, and sexual relations were reported to be satisfactory.

The quality of the parental relationship in these five cases ranged all the way from appalling (Boy One) to excellent (Boy Two). At one extreme, the father was definitely submissive and was ineffectual in his masculine role. There was no example of the other extreme, that of the domineering, overassertive, autocratic father. In between was the example of an effective, mutually respectful and cooperative parental relationship.

None of the fathers and mothers gave a history of homosexual inclinations or experiences, though they knew of this phenomenon by hearsay.

Bender and Paster (3) noted inadequate family life during the infantile period of growth in their group of 21 children who had engaged in homosexual practices, most of the parents being emotionally unstable with evidence of strife. Bakwin and Bakwin (1), however, noted no unusual family setup in an unspecified number of boys who exhibited behavior characteristics of the opposite sex early in life. Ferguson (7) gave attitude interest tests to 326 children and studied the most masculine and feminine members of each sex. Data were found to be consistent with the hypothesis that pleasant childhood experiences, enabling the child to accept appropriate models and culture patterns, lead to the acquisition of so-called normal behavior patterns, but unpleasantness causes the child to reject the normal models.

Boys and girls in lower-class urban working families were found by Rabban (25) to

be sooner and more clearly aware of their sex role pattern than children of middle class suburbanites whose fathers commuted to New York and usually arrived home from work when the child was in bed. He studied four cases intensively which represented scoring extremes in the two economic groups. When fathers spent time with their sons the boys developed a more masculine outlook than when this was not done, or when the mother was the more active person in bringing up the children.

PHYSIQUE

One could speculate on the physique of a child and its importance in helping to shape the sex role. None of the children in this series was exceptionally big and husky, though they were within the limits of normal male physical development, except for Boy One. None was sickly or unduly subject to diseases, during the period they were being seen. Only two require special mention.

Boy One, at eleven, had a height age of nine. He had inquired several times if he were a midget and said wearing high heels made him feel bigger. *Boy Four* had had a muscular impairment at birth, resulting in an inability to walk or talk until two. This condition reportedly resulted in his being pushed around and knocked down by other children.

INTELLIGENCE

The level of IQ in the five boys varied from Dull-Normal to Very Superior. The Wechsler Intelligence Scale for Children was administered, by the same person, as part of the initial workup of each case. For Boys One to Five, respectively, the Full Scale IQs were 131, 132, 80, 101, 104. There were no significant differences between Verbal, Nonverbal and Full IQs.

SIBLING SEQUENCE

Regarding sibling sequence, Koch (16) studied 384 children and noted that with spacings up to four years, second-born boys with older sisters were more "sissified" than were first-born with younger sisters, whereas when the siblings differed by more than four years the relationship between first and second born was reversed. Our findings were as follows.

Boy One was the only subject in our study found to be consistent with Koch's findings, as he was seven when his sister was born. *Boy Two* had two older brothers and no sisters, each boy being five years older than the next in sequence. *Boy Three* did not fit Koch's pattern: he was the oldest, with a sister one and one-half years his junior and a brother and sister four and six years his junior respectively. *Boy Four* was seven years younger than his sister, the only other sibling. *Boy Five* had an older brother, three years his senior, and a younger brother, nine years his junior.

FAMILY ATTITUDE TOWARDS SEX

A factor often mentioned in the etiology of this condition, as well as other abnormalties of sexual development, is the taboo on sex in the early years and the picture of vulgarity associated with it. With Boy One the child's mother had this to say about her husband. "Anything connected with a woman is dirty to him. Not only with a woman. . . . When he has to make a doody—don't touch the toilet! It's dirty, dirty, dirty! Anything connected with the toilet or the female or male organs is dirty. . . . It's only been in recent years that he's stopped putting paper on our toilet seat at home." The family of Boy Five expressed the opinion that sexual subjects should not be discussed until maturity, while the other three families appeared to have reasonably healthy sexual attitudes.

DESIRE BY PARENTS FOR CHILD OF THE OPPOSITE SEX

Hennessy (10) alluded to three of his six transvestites as having been born to parents who desired a child of the opposite sex and brought their child up as such. Among our

children, *Boy One* had been told several times by his step-parents that they wanted a boy and for that reason selected him and not a girl for adoption. *Boy Two* is the youngest of three boys in his family. While his parents may have been disappointed that their last planned child was not a girl, there was no indication they influenced him girlishly. *Boy Three*—no information. *Boy Four:* The mother said her husband had always preferred girls and wanted a second daughter, got a son but "now has resolved himself." *Boy Five's* mother confessed, in a self-blaming way, that she had prayed for a girl, had told the the doctor to take the boy back, but then "wouldn't trade him for the world."

Whatever their hidden desires, the parents of these boys brought their sons for study, it should be noted, because their explicit desire was to rear a boy sexually normal in mannerisms and behavior.

MOTHER-IDENTIFICATION

Mother-identification as a result of a weak father, strong mother combination, or for other reasons, has sometimes been pointed to as the chief factor in feminine orientation in a male. It was of interest to observe indices of identification between son and mother in in Boys Two and Four. Boy Two's mother had a minor speech defect in pronouncing the letter "s" while her son had the identical defect, greatly magnified. Boy Four had a peculiar way of screwing up his face while speaking in a whiny voice, and one only had to observe his mother once to note that the boy's actions were a caricature of his mother's facial and vocal actions. No obvious similarities of behavior were observed with Boys One or Five, and no information is available about Boy Three, since his psychotic mother could not be interviewed.

GENDER-APPROPRIATE PLAY ACTIVITIES

The boys we studied showed either a preference for, or ambiguity towards, choice of gender-identifying playthings and activities. To what extent has it been ascertained that ordinary children show gender preferences in play?

Brown (4) found that boys between the ages of five and one-half and seven and one-half already show a predominantly masculine-role preference. He based his conclusions on a series of pictures illustrating toys, clothing, cosmetics and people shown to 613 children between five and one-half and eleven and one-half. When boys of all grades were combined, about four per cent responded with an unequivocal feminine preference, and about 63 per cent with an all-out masculine preference. The younger boys showed a much stronger masculine-role preference than the girls did feminine but, after the age of 11, the girls' preference became predominantly feminine.

Murphy (23) noted that up to age four there generally is no sex differentiation in play, but at four and five boys identify themselves with a masculine role. However, Koch (15) mentioned that at age two nursery school children are inclined to favor classmates of their own sex, which might suggest gender identification at this age. Miller (19) found that at age three children were aware of the inappropriateness of boys playing with dolls, but not before.

Rabban (25), using dolls, had children select one like themselves and one of the opposite sex; he also asked them if they eventually wanted to be a mother or a father. He found that, at age three, boys and girls showed incomplete recognition of sex difference in dolls, or in themselves, and that many voiced preference for the opposite-sexed parent role. At four there was virtually perfect gender-identification except for some cross-parent preference. Overall differences between boys and girls were particularly significant in the fifth and sixth years, with earlier and clearer awareness for boys.

AWARENESS OF SEX DIFFERENCES

During our interviews, inquiry was directed to the child's awareness of the difference between the sexes and what concep-

tion he had of where babies come from. Without an awareness of the inalienable differences between male and female, as opposed to the more easily surmountable differences of dress and haircut, a child might readily conjecture that a change of sex can easily take place. The infantile logic of losing a penis and becoming a girl, or gaining one and becoming a boy is quite as feasible as the logic of gaining breasts to become a woman, or a beard to become a man. While far from universal this kind of infantile logic might well apply in certain special cases.

There is the possibility that a child who has had the opportunity to see the genital differences between the sexes might repress, distort or fail to generalize his observation. Boy One insisted his sister had a penis. Boy Four claimed his sister had the same thing as he to "make sissy," but later allowed that only some girls have a "tushy" like a boy. Boy Three thought "everybody had a penis" except his baby sister and he "didn't know why" she didn't. Boys Two and Five had accurate conceptions of sex differences. Only Boy Two knew about the penis in procreation, but had it being put in the "belly button." Boy Four, the youngest, alone failed to report that babies come from the mother. None, except Boy Two, had an adequate conception of delivery. Boy One thought delivery was rectal, Boy Three that it was through the breasts, and Boys Four and Five reported they did not know.

Conn (5) found that half of the children he interviewed between the ages of four and six had knowledge of the difference in the genitals. By 11 to 12, only 14 per cent were still unable to state the difference. Interestingly enough, in the youngest group, 17 per cent more children were known to have witnessed the difference than were able or willing to tell about it. Katcher (13) found that of the boys he saw, aged five to six, one half correctly identified a pictured female genital. He found no significant difference in this ability between one- and two-parent children and, furthermore, there was no particular observed advantage for those children with opposite-sexed siblings.

PSYCHOLOGIC HEALTHINESS APPRAISAL

In the past, gender-role disturbances in childhood, without additional signs of behavioral disturbance, have ordinarily been minimized. It required the presence of other symptoms before the child came to the attention of a psychiatric clinic or law-enforcement agency. In the present study, there was only one child who had such additional symptoms. This was Boy Three, a forlorn and lonely child, profoundly affected by his separation from his siblings and his hospitalized schizophrenic mother. Clinically he was depressed, but not so profoundly as to require hospital care. He was enuretic and apparently had been so all his life. Probably it was his enuresis more than his alleged lack of masculine ruggedness that made his house-proud foster mother want to return him to the welfare agency.

Boy Three had a profoundly disturbing domestic history. So did Boy One and, to a less persistent extent, Boy Five. It could easily be said of these latter two that they were happy, well-behaved and well-adjusted, if one were ignorant of their effeminate interests and disinclination to be in the rough-and-tumble of boyhood. Their effeminacy appeared to be the only odd feature of their generally good adjustment. The same is true of Boys Two and Four, whose home life was good.

PROGNOSIS AND THERAPY

What are the prospects of gender-role reorientation for these children? At this time, only Boy One has been followed extensively. Four years after the first visit, this youngster was still conspicuously effeminate in gait and gesture. He had put away his dolls and his father was no longer wheedled into buying them for him. He "dressed up" only rarely, but had the urge to do so frequently. He was always the first to primp in his sister's new clothes. She was a tomboy. The parents

still barely tolerated one another and had not had intercourse for three years. The father willingly acquiesced to the boy's demand to be put to bed. The father went to sleep lying beside his son and usually stayed there all night. He was intensely devoted to the boy, and declared with great intensity that he had never loved anybody or anything as he loved him. Though the boy had been willing and cooperative in treatment, his effeminacy was basically unchanged.

It is too early to make any definitive general statements about the outcome of effeminacy in early boyhood. It appears, however, that there are some cases in which the condition is so firmly entrenched by middle childhood that it cannot reasonably be expected to disappear. Possibly there is a critical period in the earlier years in which effective intervention is possible. Otherwise, the therapeutic aim must be to ameliorate the condition as much as possible, especially in its socially conspicuous manifestations, and to do what is essentially a job of rehabilitating a chronically handicapped juvenile so that he achieves as successful and productive an adulthood as possible.

DISCUSSION

A review of the foregoing sections reveals no clear-cut etiological factors. There is, indeed, a notable variability from case to case. The most consistently recurring observation is the infrequency of paternal forceful dominance in the household—not uncommon in contemporary America—and the frequency with which the mother views the son's behavioral anomalies more seriously than the father does. No variable, singly or in combination, points the way to a likely explanation of the etiology of effeminacy in a young boy. Etiological explanation must at the present time, therefore, be conjectural.

One conjecture might be that disorders of gender role and orientation are determined by a genetic factor. It has been proposed that transvestites and homosexuals have a female genetic constitution. This once popular proposition has not been upheld by recent work on nuclear sexing—Barr and Hobbs (2), Pare (24). Nor has it been upheld by the evidence of adaptation of hermaphrodites to the sex of assignment at birth (21).

Gender role and orientation may well be established in the way that language gets established, involving a multiplicity of factors, some of which apply to one individual, some to another. Thus, one child assimilates the accent of his parents, another that of his playmates; one child becomes bilingual, while another repudiates the foreign tongue native to his parents.

The critical period for gender role anomalies to become ingrained appears to be very early in life. In the hermaphrodite studies above mentioned, it was found that of nine children reassigned sex below the age of two years and three months, six made uneventful adjustments, while in the group reassigned later in life only one out of five met with similar success (9). The importance of the early years is apparent in our cases, also, in the early age of the onset of symptoms. Boy One was carrying a pocketbook as soon as he could walk. A neighbor of Boy Five noticed feminine mannerisms "from a baby on up," and the mother was acutely aware of the situation before the age of five. Boy Four's activities brought notice at about age four. There are no data for Boy Three, leaving only Boy Two as the Johnny-come-lately of the group, with no one noticing much effeminacy until he was about six.

It is possible that the timing of more or less fortuitous events in early life contributes to the development of effeminacy. The two boys with older brothers were Boys Two and Five. These brothers were not in the least effeminate. Boy Two's life experience was different from his brothers' because he had had an aunt as a constant and lavishly affectionate companion from birth to five, while his brothers had not. Boy Five was three when conflict between his mother and father broke out, whereas his brother was already six.

Effeminacy in boys may be viewed as the product of identifying with and impersonating the wrong sex. In this case, Lorenz's imprinting concept may be offered as a paradigm of what happens in human beings as they develop a gender role and orientation. Imprinting is a kind of learning that takes place only at a specified and critical period in the life history. It is triggered by a specific perceptual stimulus which can be varied, but only within certain boundaries. Aberrations of gender role may represent misprinting, so to speak, in which a more or less normal response, that of identifying with and impersonating a specific human being, becomes associated with the wrong perceptual stimulus. Among animals good and poor imprinters can be bred (11). Perhaps, therefore, those human cases of gender-role disorder which come to our attention are examples of people especially prone to fall victim of their particular environment.

The foregoing brief speculation aside, two things are clear. First, incongruous gender orientation is recognizable in small children and may not be merely a "cute" transient phase in the child's development. To wait until adolescence to pay it serious attention is to give this gender misorientation more time to affect permanently the behavior of the individual.

Second, part of the successful rearing of a child is orienting him, from birth, to his biologically and culturally acceptable gender role. This, as far as we know, is best achieved by providing a relationship between husband and wife exemplifying these respective roles.

SUMMARY

Five prepubertal boys who at the time of their referral were assuming many of the behavioral characteristics of girls, have been studied. Representative excerpts from their transcribed interviews as well as other data are presented under the following subheadings: presenting symptoms; age at onset of symptoms; stated desire to be of the opposite sex; parental relationship; physique; IQ; family attitude toward sex; sibling sequence: desire by parents for a child of the opposite sex; mother identification; gender-appropriate play activities; awareness of sex differences; psychologic healthiness appraisal; and prognosis and therapy. No one variable was found to be consistent with the etiology of gender-role disorder among these children. A theoretical discussion of the findings is offered with some reference to the concept of imprinting as it may apply in the early establishment of inappropriate gender role.

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BRIEF COMMUNICATION

SOME CONSIDERATIONS OF PARANOID SCHIZOPHRENIC-PASSIVE DEPENDENCY MARRIAGES

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Recently increasing attention has been paid to the concept of personality interaction in marriage (1). During the past several months we have seen, because of a psychotic break in the wife, four sets of marital partners in which the wife was a paranoid schizophrenic and the husband a passive dependent personality (4).

Some questions raised with regard to this marriage combination are:

1. What part does the personality of each partner play in the other's psychic economy?
2. Of what significance is it that in all our cases the wife was the psychotic partner?
3. What role did the husband's personality play in initiating or in facilitating the wife's psychotic breaks?

CASE REPORTS

Case # 1 is a 38 year old white female who since September 1958 has been hospitalized three times with a diagnosis of schizophrenia. Two hospitalizations ended when at her insistence she was signed out by her husband against medical advice. In November, 1959, her husband appeared at our clinic and stated that since leaving the hospital two months before she had again grown increasingly suspicious, seclusive and demanding. Her husband had acceded to her demands to an extraordinary degree, and it was only when her accusations began to involve the neighbors that he came for help. The husband was a flaccid, clinging individual who related in a dependent manner. When told he might have to bring his wife where she could be seen, he pouted like a petulant child and refused to take any action. At this point a decision was made to send the psychiatrist to see her at home. When seen the patient was agitated and related distantly with a grandiose, accusatory air. She was preoccupied with the idea of having insufficient clothing because of neglect on her husband's part, and made remarks like "he brought me down here with only two pairs of pedal pushers." Her husband replied to these accusations in a child-like pleading tone that he had given her all the clothing she wanted, and would give her more if she wanted it. She would not accede to her husband's servile pleas that she go to the hospital, but with firm insistence she went voluntarily.

Case # 2 is a 26 year old white female who in 1953, after the birth of her third child, became withdrawn and suspicious, and was hospitalized on several occasions over a period of eight months. She apparently functioned well thereafter until November 1959 when she was hospitalized and operated on for a right ureteral stone. Five days postoperatively she became mildly agitated, spoke of past wrongs, and refused to follow necessary instructions. The surgical personnel tended to acquiesce in her refusals, and tried to calm her by letting her talk freely. She became more and more agitated, and two days later a psychiatric consultation was requested. When first seen the patient had a clear sensorium, with blunted affect, and related in a hostile, suspicious manner. She showed moderate paranoid ideation concerning ward personnel, and seemed preoccupied with "things done wrong in the past" and "guilt." She was placed on chlorpromazine and was seen daily while in the hospital, until her discharge in four days.

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Both she and her husband were seen the day after discharge. History obtained at that time revealed that throughout the course of their marriage the patient had tended to assume the majority of the responsibility for running the household, and to do most of the decision-making. The husband was noted to be a passive, clinging individual, whose major concern seemed to be restoring the *status quo* of having his wife run things. One gained the impression that the stability of the marriage rested on such a relationship which was changed only during the wife's periods of active psychosis.

DISCUSSION

Our tentative answers to the above questions are as follows.

1. The paranoid schizophrenic wife is, as has been noted (3), an individual who deals with unacceptable and hard to control impulses by projecting them to the external world where she can control them by utilizing "mechanisms of protection against overly intense stimuli (which) can be set in motion against external stimuli only" (2). In order to then control these impulses the person so projecting must be able to achieve a high degree of control over the external world. A passive, dependent individual (or, as a patient described her husband, a "cute . . . little man") offers the paranoid schizophrenic a marital partner and life situation in which she can do this. In all four cases—of which two have been briefly summarized—the wife had assumed control of the family to a degree which would have been impossible with a more assertive partner, and this seemed necessary for her effective functioning.

What the wife gives the husband is somewhat more obscure. In all of these cases the husband seemed to seek domination by the wife. However, if he is looking for a mother figure, the wife here lacks the maternal attribute of giving. We can only speculate that the husband is seeking a maternal archetype with qualities of domination and giving, and that what attracts him here is the domination, with the hope that with this will come the giving.

2. In each of our cases the husbands were members of the military, the wives civilian dependents. Since members of the military with psychosis are separated from the service, we assume this accounts for the fact that the wife was always the psychotic partner. It is possible, however, that factors inherent in the relationship would tend to favor this combination, and this might be checked in a civilian facility.

3. All of our patients were judged to be psychotic when they came to our attention, and they had had among them four previous psychotic breaks. In none of these were we able to identify any particular change in the marital relationship which occurred prior to the onset of the psychosis, and could find no other evidence that the husband's personality played a part in the inception of the break. In distinction to this we feel that the husband's personality played a definite role in facilitating (or failing to retard) the psychotic break once it appeared. In each case the wife apparently began to experience a resurgence of impulses which she was unable to deal with entirely by projection, and therefore began to sense as uncontrollable threats from within. As one of our patients stated, "This morning I got to laughing again and talking to myself out loud, of all things. I just don't understand it, it seems I have no control of myself." At this juncture the individual, having failed to establish control of her impulses by previously effective means, seemed to turn to the external world. There, by using unreasonable demands and actions she seemed to seek external limitations which would establish control for her. With this change the husband's pliability and acquiescence, heretofore assets, resulted instead of in limitation, in a yielding to her demands and actions. The patient then felt even more threatened by her impulses, and it seemed to be at this

point that serious impairment of reality testing ensued.

SUMMARY

Four cases of married couples in which the wife is a paranoid schizophrenic and the husband a passive dependent personality have been seen in our clinic in the recent past. Two of these cases are briefly presented, and some questions arising from such a marriage combination are discussed.

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DELAY, J., PICHOT, P., LEMPÉRIÈRE, T. AND PERSE, J. *The Rorschach and the Epileptic Personality* (BENTON, ARTHUR L., ED.). Logos Press, New York, 1958. xx + 265 pp. $6.00

Like an old family ghost the image of the epileptic personality does not quite come to life, yet manages to retain some sort of insubstantial, ambiguous identity. The book written by Delay and his colleagues is vivid testimony both to the fascination that this alleged personality syndrome holds for clinicians and students of behavior, and to the scholarly competence with which the authors are able to summarize the present status of the epileptic personality. Although this is a dedicated work, it embodies a realistic awareness of the tentativeness and instability of our knowledge of this subject. The authors express this awareness in the following remark. "For some, the epileptic personality does not exist, that is to say, it cannot be differentiated in any way from the mental disturbances associated with cerebral diseases in general; for others, it is characterized by special traits, which are found even in the families of epileptics. For some, it is related to idiopathic epilepsy; for others, to the temporal epilepsy. For some, it is innate; for others, it is acquired" (p. xx). In addition to its general appeal to all students of the epileptic personality, regardless of field of specialization, this volume is of particular interest to all clinicians and researchers who use the Rorschach in their investigation of this problem area.

In the foreword the authors state their intention of presenting a summary of "current conceptions of the epileptic personality" and a review of the Rorschach literature on this subject, before reporting their own study. As it turns out, 172 pages—of a total of 253—are given to the review of the literature; the report of the original investigation is presented in the remaining 81 pages. This distribution of content is pointed out not to diminish the stature of the original study, but rather to emphasize the comprehensiveness with which the relevant literature is critically reviewed.

Were it not for the summarizing and systematizing tour de force executed by the authors, reading the review would be a most discouraging enterprise. As one proceeds through the first chapter on "The Epileptic Personality," one perceives rather early a babel of discordant terminology and concepts, an esoteric impressionistic trend in clinical formulations and an unbroken sequence of contradictory findings. Undismayed by this farrago of theories, beliefs and impressions, the authors rather neatly categorize the various etiological theories into the following groups: 1) hereditary psychological traits of the epileptic constitution; 2) results of post-traumatic epilepsy; 3) reactions to painful social situations; 4) reactions to latent homosexual tendencies; 5) primitive instinctive drives released by the dissolution of higher functions following cerebral lesions; and 6) nonconvulsive forms of epileptic seizures, particularly of the temporal kind. In what may appear to some readers as a *non sequitur* to the above summary, the authors then express their optimistic belief that it is possible to study the epileptic personality experimentally "to a certain degree," and their faith in the Rorschach as the instrument of choice for such a study.

The second chapter embodies a fundamental contribution to clinical psychology in its critical summary and review of the total extant Rorschach literature on epilepsy. This chapter reports the analyses of 77 studies extending over a thirty-year span, from 1925 to 1954. Problems encountered by the authors in completing this summary are well reflected as follows. "It is rather difficult to abstract this literature, not only because of its extensive character, but also because of variations in the procedures and

scoring systems employed and the heterogeneous character of the case material" (p. 731).

This reviewer would not recommend the sequential reading of the 135 pages of this chapter, for he can report that the experience is both confusing and depressing. The authors are well aware of the defects to be found in this massive array of studies and clearly point out the many inadequacies in their critical summary. As a first step, 34, of the 77 studies reported, were eliminated from the final summary because of gross defects apparent to even a superficial observer.

About the remainder the authors have the following criticisms: insufficient subjects, especially in subgroups; subgroups not matched for intelligence and intellectual deterioration; absence of statistical tests of significance; samples limited to institutionalized epileptics; failure to distinguish between various clinical types; and failure to use control groups. The inadequacies of these studies are, of course, not the responsibility of the authors. Their task in summarizing and evaluating them was a herculean one, and has resulted in the concise and most valuable assembly, within a single chapter, of all the Rorschach work done within this particular area of inquiry.

In their own investigation Delay *et al.* examined 50 ambulatory epileptic patients in whom the diagnosis was confirmed both on clinical grounds and by electroencephalographic study. If one applies their own standards to their sample and research design, the following points of congruence and divergence are worthy of note. On one hand, Delay and his colleagues went to considerable length to accurately classify each subject in terms of type of etiology (symptomatic-traumatic, symptomatic-nontraumatic, essential epilepsy, cause unknown) and localization (temporal psychomotor, temporal non-psychomotor, other localization); they had accurate measures of intelligence; the subjects were all ambulatory and therefore more representative of the general epileptic population than those often reported in previous research. On the other hand, when the 50 subjects are assigned to the various subcategories one finds some numbers as small as one and two, and none larger than seven. Although the authors consider that such measures as Pietrowski's Organic and Epileptic Signs have normal referrents, the fact is that no control groups as such were used. In analysis of the data, the authors do apply statistical tests, such as the "t" test for significance of differences between subgroups and both chi-square and tetrachoric correlations for the relationship between the various measures applied to the Rorschach data. Every Rorschach response was scored in four ways; by using the traditional determinants, Pietrowski Organic Signs, Pietrowski Epileptic Signs, and Minkowska's signs. The reader will readily recognize the first three scoring procedures. The last is not generally known in this country and indeed its meaning as outlined in this book is rather ambiguous.

In summarizing their findings Delay *et al.* assert, "There certainly exists an 'epileptic personality' in the sense that all epileptics present some common traits, whatever the etiology or clinical form of the disorder. These are: reduction of creative intellectual productvity (which is independent of reduction of intelligence level) and of the quality of this productivity, reduction of imaginative capacity leading to ideational perseveration with relative awareness on the part of the patient of his incapacity, and a lack of confidence in his productions. These traits are, however, those of the personality of organic patients in general and are not specific to epilepsy.

"Superimposed on this basic personality there appears another modification, the character of which varies as a function of etiology and localization, and which is related to the degree of the social adaptation of the subjects. The idiopathic epileptic

tends to have a coartative personality which, in restraining his emotional reactivity, permits him to make a satisfactory social adjustment. In contrast, the traumatic and psychomotor epileptic tends to present a strongly extratensive personality, with excessive and unadapted emotional reactivity, leading to social maladjustment related to personality difficulties of the impulsive-explosive type. This opposition is only schematic and the subjects distribute themselves between the two extreme poles" (p. 233).

This reviewer cannot refrain from expressing some disappointment in the overall findings of this elaborate and carefully conducted investigation. The results do no more than iterate—perhaps in somewhat more specific terms than is usually the case —what has been generally known about various trends in the personalities of people who have epilepsy. The proof in this study regarding the relationship of specific personality deviation to etiology and localization is not very convincing, especially in view of the small number of subjects in the subgroups. The conclusion that the epileptic personality is in many respects similar to the organic non-epileptic personality is, of course, also consistent with previous knowledge. In view of this similarity between epileptic and non-epileptic organic patients it is difficult to understand the rather exuberant claim of the authors regarding the usefulness of the Rorschach in the differential diagnosis of epilepsy. It need hardly be added that this study adds nothing to the settlement of such controversial questions as the hereditary or acquired nature of the epileptic personality and its reactive or primary role in relation to the disease.

Nevertheless, if one is interested in a description of the kinds of personality trends that are likely to be encountered in epileptic patients, this book adds considerable descriptive detail, rather more carefully classified and organized than is found elsewhere. Moreover, it is a meticulously organized abstracted report of the current state of knowledge regarding the epileptic personality, both in terms of general theory, and more specifically in terms of Rorschach findings. No one working in this area can afford to overlook this volume.

Benjamin Pope

MILLS, C. WRIGHT. *The Sociological Imagination*. Oxford University Press, New York, 1959. 234 pp. $6.00

This is a book intended for the non-sociologist as well as for the social scientist. It is not, as the title implies, an introduction to sociological thinking but a polemical critique by a maverick sociologist of some current major tendencies in American social science.

According to Mills, the "sociological imagination" has two aspects. Its scientific function is to redefine personal troubles and anxieties as public issues and to study them as basic problems of social structure and social institutions rather than as problems of discrete, unrelated social milieux. Its moral function is to act as an intellectual clarifier and critic of our modern cultural and political life; a completely "objective" role implies moral irresponsibility. Mills claims these functions are best embodied in the classical sociological tradition with which he identifies himself, but believes they are threatened by three contemporary trends in the social sciences.

One is "grand theory," which refers to the voluminous writings of Talcott Parsons and his students. "Grand theory" is caustically attacked and cavalierly disposed of in twenty-five pages; it is accused of questionable intelligibility and when rendered intelligible (as in Mills' ironic translation of *The Social System*, which he reduces from 555 pages to four paragraphs) of being useless and irrelevant with respect to the two functions of the "sociological imagination." "Abstracted empiricism," Mills' jargon for the quantitative approach in sociology in which Paul Lazarsfeld and survey meth-

odology are singled out, is said to be incapable of handling concepts and problems of social structure, as well as of depth psychology. No useful scientific function is seen for methodology as a specialization within social science. "Illiberal practicality," the third trend, refers to the ideologically conservative role that Mills claims is played by social scientists, in and out of universities, who increasingly perform work for industrial and military organizations, as in the "human relations in industry" approach.

Mills believes these trends inherently support, directly or by default, the development of a bureaucratic social science wherein the research problems are dictated by administrators, the intellectual operations are standardized, and the solutions primarily strengthen domination over society by large hierarchical organizations. The social scientist, geared to these trends by his training, loses his scientific and moral autonomy.

The book succeeds in raising significant problems and questions neglected by many sociologists about the place and function of social science in our present society. This neglect occurs despite (or perhaps because of) increasing concern by sociologists, individually and collectively, with their professional status. These questions have even greater current relevance and urgency if one agrees with Mills that the social sciences are already displacing the physical and biological sciences and the humanities as the major common denominator of our intellectual and cultural life.

However, the argument of this book, painted in broad unyielding brushstrokes, obscures important details and results in unjust distortions. In constructing pure types of schools within sociology, *e.g.* "grand theory," and in implying their mutual exclusiveness, Mills may only succeed in conveying stereotypes to the many non-sociologists who will read the book. He ignores many significant instances of mutual influence among these "schools." One example is Parsons' work with *both* classical sociological writings and "grand theory," as well as his use of "grand theory" to analyze major contemporary social issues (a task which according to Mills is only performed effectively in the classical tradition, *e.g.*, his well-known essay on aggression in Western society). Another example illustrating mutual influence is the use of the techniques of "abstracted empiricism" to test and reformulate classical and "grand" theories. (One can demonstrate this in at least several, perhaps a half dozen, of the dozen social scientists praised by Mills as "excellent" on page 95). The descriptions of these pure types are more often attempts at caricature than serious comprehensions; isolated, extreme and vulnerable expressions of one or two representatives of a "school" are often selected as straw-men illustrations. Mills' personal bias is often apparent, as in his frank disdain of systematic empirical work in the Appendix on "intellectual craftmanship."

Indeed, this book throughout lacks evidence to support or illustrate its argument. In ten pages (pp. 65–74) devoted to four or five criticisms of "abstracted empiricism," not one example of research of this type is cited or analyzed. Similarly, in alleging an increasing subservience of academic social science to conservative ideology and bureaucratic authority, pertinent evidence on the actual work of university research bureaus and departments is not examined, nor are available figures used on possible changes in sources and amounts of research funds.

Mills identifies the important problem of the social scientist's autonomy in a hierarchical organization, but unfortunately oversimplifies it. He does not consider other elements in society that may modify or curtail the authority of bureaucratic organizations over individuals or groups. Thus he does not examine the academic social scientist's area of control over research sponsored and even defined by outside organizations. Nor does he explore the impact of

social scientists working in practical administrative settings on the policies of organizational elites; the social scientist working in medical and psychiatric settings offers a complex example. Much that is written about the causes and consequence of large-scale organization of any intellectual endeavor lacks empirical support: Mills assumes they are inherently uniform and unidirectional but these are open questions requiring closer examination.

Granted these shortcomings, the book is nevertheless provocative and stimulating. Non-sociologists, including workers in the mental health field who have contact with social scientists or their writings, may gain further insight into some controversial aspects of the sociologist's professional orientation. Although directly concerned with the sociologist, the book has clear implications for all who study and control human behavior.

Joseph Greenblum

KOOS, EARL L. *The Sociology of the Patient*, 3rd ed. McGraw-Hill Co., New York, 1959. xi + 266 pp., illus. $6.00

This book is intended primarily as a textbook of sociology for professional nursing students and is not likely to be of interest to persons in the psychiatric field unless they happen to be engaged in teaching nurses. The title may be misleading to sociologists since the book is not specifically about the sociology of patients, unless one regards mankind simply as a group of patients.

The author states that he has been teaching nursing students for a quarter of a century. He apparently has developed a very low opinion of the intellectual ability and worldly sophistication of nursing students. For example, when referring to man as a member of the animal kingdom, he finds it necessary to remind his readers that "it is no reflection upon man to speak of his being a member of the animal kingdom, for he shares with the lower animals many characteristics" (p. 1).

The bulk of this text is a presentation of elementary sociology—and a more elementary presentation of sociology this reviewer has never seen. The author covers a wide range of topics, including an overview of man's prehistory and evolutionary development, culture, institutions, learning (at least one theory thereof), socialization, the family, social interaction and stratification. Despite the 1959 copyright date, most of it could have been (and perhaps was) written two or three decades ago. The more recent research and critical discussion in the field of general sociology is almost completely ignored. Thus the topic, "Interaction Among Groups" consists entirely of a brief discussion of assimilation, accommodation, competition, and conflict. The "Structure of the Urban Community" is explained entirely in terms of the concentric zones of a city which were originally presented by Park and Burgess. Such concepts are important and still have considerable usefulness but to present these topics entirely in these terms without paying any attention to more recent work or critiques gives the present-day student a false and incomplete picture of the state of knowledge and thinking in these fields. True, an introductory work, especially for students not specializing in the field, cannot be a comprehensive scholarly treatise. However, if a book is to be of any use at all in increasing the knowledge and broadening the intellectual perspectives of a student, there must be reasonable limits placed on the simplification of ideas and subject matter, and recent research work that is relevant to the field of study should not be ignored. In the opinion of the reviewer Koos is deficient in all these respects.

The last four chapters (out of eleven) relate somewhat more directly to the fields of health and mental health. Here the book seems more up to date, especially in the demography of health and disease and other health statistics, which clearly are germane to the 1950s. In other respects, however, the last chapters again explain human behavior

in terms of a grossly over-simplified social psychology. Types of mental illness are presented according to the traditional categories (with a sharp dichotomy into "organic" and "functional" diseases) which have proved of little help to psychiatrists and less help to sociologists, and which are not likely to have any effect on nurses unless it be to cause them to stop doing any thinking at all about the dynamics of psychiatric disorders. These pages contain many superficial and naive statements, as for example when the author lists such things as thumb sucking, masturbation, shyness, rebelliousness, and sex activity as "problems" which may lead to mental disorder if not properly treated in childhood.

It is difficult to imagine a situation wherein this text would be useful. If a teacher of nursing students knows his field fairly well, he would do better without a text—certainly much better without this one. If he knows little or nothing about his field and is therefore forced to read a text along with his students, he would do better with almost any other introductory sociology text picked at random—even one published several decades ago.

Julius A. Roth

RINKEL, MAX AND HIMWICH, HAROLD E., EDS. *Insulin Treatment in Psychiatry.* (Proceedings of International Conference on Insulin Treatment in Psychiatry, New York Academy of Medicine, 1958) Philosophical Library, New York, 1959. xxix + 386 pp. $5.00

This book is a report of the proceedings of the International Conference on the Insulin Treatment in Psychiatry held at the New York Academy of Medicine in October, 1958. The volume consists of sections on historical contributions, physiochemical research and clinical research and follow-up. In addition there are stimulating discussions following the more formal papers and a summary of the entire conference. It was the stated purpose of the conference to assess the value of insulin treatment of psychoses, to provide a global view of the use of such treatment, to learn about its failures as well as its successes and to compare its results with those of other therapies. Perhaps the most striking feature of the book, however, lies in the degree to which it serves as a testimonial and historical tribute to the discoverer of insulin coma therapy, Manfred Sakel.

It is difficult to assess the value of the book for psychiatry in general. Practitioners who continue to utilize insulin in the treatment of schizophrenia cannot fail to be impressed with the frequent reiteration that the evidence demonstrates that insulin therapy is not outmoded and is still essential in psychiatry. Advocates for combined therapy in the treatment of psychoses may derive some practical value in the numerous claims for effective results when utilized in combination with electric shock, individual and group psychotherapy and drugs. To those interested in the larger questions of etiology in schizophrenia, the specific mechanisms of insulin, the problem of diagnosis in relation to insulin therapy and the problem of defining and developing adequate criteria for assessing therapeutic change the volume provides provocative discussion. One can only agree with the authors and participants who stress the need for continued research in the important realtionship between biochemistry, physiology and psychiatry. If this volume succeeds in stimulating such efforts it will serve a most useful function. In the absence of systematically controlled experiments coupled with astute clinical observations insulin shock therapy as well as other organic treatments of schizophrenia will continue to remain largely on a trial-and-error basis.

Melvin Rosenthal

HARTMANN, HEINZ. *Ego Psychology and the Problem of Adaptation.* International Universities Press, New York, 1958. xi + 121 pp. $3.00

This monograph was written by Dr. Hartmann in 1937, at which time it was pre-

sented before the Vienna Psychoanalytic Society. It was later published in 1939 in the Internationale Zeitsschrift fur Psychoanalyse und Imago, with the title "Ich Psychologie und Anpassungsproblem." This very important work, which brings forth or foreshadows so many of the important concepts which are farther developed in Dr. Hartmann's later contributions, has not been available in English until the present time. It now appears in translation and essentially unchanged, according to the author. The difficult task of translating a work of this nature has been very well handled by Dr. David Rapaport who himself is an authority on ego psychology and the problems of adaptation. As attestation of the great value of this work, it was given the Charles Frederick Menninger award of the American Psychoanalytic Association. This monograph is extremely rich in content and touches upon every aspect of adaptation. The material encompassed could well have been expanded into a book several times the present size. As a result it cannot be read with great ease and rapidity but demands and deserves careful study. Every page in the monograph includes several distinct ideas or suggestive areas of research and investigation which point toward further contributions in the field of psychoanalysis as well as in fields of education and sociology.

The central point of interest is the problem of adaptation, which involves biology and sociology as well as psychology. When psychoanalysis began to study the functions of the ego and the total personality it became a general psychology in the widest sense. The problem of adaptation became important in psychoanalysis as a therapeutic aim, as a problem in Ego-psychology and in the study of education. While the exploration and study of the id continues to be the particular preserve of psychoanalysis, its increased accentuation of the study of the ego provides an area where it meets and comes in contact with non-analytic disciplines. Future development will make it essential to adapt psychoanalytic concepts to other realms of mental life so that the changes in these concepts necessitated by the conditions prevailing in these other areas can be studied.

Inasmuch as psychoanalysis developed as a result of therapeutic aims it has naturally placed a major emphasis upon the study of conflict in all of its aspects. At the same time, there is a large ensemble of functions, basic roots of ego development, which lie outside the region of conflict. Dr. Hartmann speaks of this ensemble as the conflict-free ego sphere. In order to see the relationship between these two areas we must study the interaction of mental conflict and personal internal development as they interact and shape the internal world. Conflict does not necessarily signify pathology and defenses may lead to pathology. Intelligence, learning to think, intellectualization of instinct, etc., must be seen as basic components of the ego rather than treated as ego functions. In order to assess the strength and stability of the ego it is necessary to consider the role of the non-conflictual areas and the indirect effects of the defenses. Ego development must be studied in relation to such non-conflictual areas as intelligence and motor ability. It is essential to recognize the differences that exist between the functions of fantasy in the child and in the adult. Likewise we must differentiate between reality testing and reality adaptation and recognize that such functions as fantasy, denial, avoidance and the affects may be useful in reality adaptation.

Among multiple problems included in the concept of adaptation Dr. Hartmann stresses productivity, ability to enjoy life and mental equilibrium. They must be judged in relation to typical life situations and not to the unusual or atypical experience. Adaptation to a typical situation is to a great degree guaranteed by the primary equipment of the organization and its course of maturation, including the development of ego-regulated functions. Although the

genesis of various functions of behavior remains important, this monograph is mainly concerned with relationship to adaptation. Thus defenses may become fixed and represent an important goal in themselves. Adaptation involves autoplastic or alloplastic changes as well as the more radical action of seeking or fashioning a new environment. There may be marked conflict in terms of adaptation between the needs of the social group and the individual.

In the consideration of the problem of adaptation it is fortunate that Freud built his theory of neurosis on biological rather than strictly human conditions. He stressed the prolonged helplessness of the infant, the presence of a long latency period and the fact that the ego treated certain instinctual tendencies as being dangerous. Because of man's meager instinctual equipment, learning is his main tool in the furtherance of adaptation. The processes used in adaptation are determined by constitution as well as environment and are apparent in the ontogenetic development. Solutions taken over from history and tradition enter into both the ego and super-ego. Inasmuch as the social structure within which the individual lives determines his chances of success in adaptation, Dr. Hartmann introduces the term "social compliance" as a counterpart of a similar concept, namely somatic compliance.

Dr. Hartmann points out that we cannot make an antithesis between biological and environmental factors inasmuch as psychological factors are as truly biological as organic. Instinctual drive as considered in psychoanalysis comes in between the psychological and organic. Thus our useful area of consideration is in relation to such terms as congenital, maturational or environmental. We likewise must evaluate whether the exogenous factors are the average expected kind or exceptional in nature. While we are quite aware of the role of progressive development as useful in adaptation, we must not overlook the fact that regression may serve a similar purpose. This concept was developed by Dr. Kris as regression in the service of the ego.

The individual must maintain an equilibrium with his environment, which may be either normal or pathological in nature. The three main approaches to this problem lie in the relationship between the individual and the external world, the relationships between the instinctual forces, and last, the relationship within the structural system. But we can also consider a fourth factor, the synthetic function of the ego, which is really a specific organ of equilibrium. Correlation of the individual parts of the organism lead to fitting together and adaptation. With increasing internalization the synthetic functions of the ego become the psychological expression of these functions. The synthetic function holds a higher position in the hierarchy than does adaptation because it deals with both internal and external functions. Although the pleasure principle serves self-preservation, it is the reality principle which brings about anticipation of the future and relates means to ends, which themselves are important ego functions. Dr. Hartmann raises the question as to whether we should use the term "reality principle" in a much broader sense and include not only factors related to the individual but those which are related to the whole race (such as natural selection).

It is important to differentiate phylogenetic from ontogenetic factors. While there are no well-defined instincts in man that guarantee adaptation, the entire psychic structure has survival value in an average environment. The new-born infant has inborn defensive processes as well as instincts, which act as regulating factors, that are later replaced by new apparatus on higher levels in accord with the development of the ego, in turn partly based upon constitutional factors. Thus the new-born is in a state of adaptiveness to a typical environment.

Structural development serves adaptation

not only through the ego but through the superego, which is to some extent in itself the ideal prototype of the pattern or state toward which the ego's endeavors are directed. Increased differentiation leads to greater possibilities for adaptation provided it is paralleled by the development of synthetic capacities. Adaptive mechanisms such as full compliance and many neurotic formations may actually lead to disturbed adaptations.

We attribute a certain objectivity to that reality to which the individual adapts. If the adaptation is in relation to a goal it is related to the ego; if the adaptation is related to values it is a function of the superego. Rational goals are not necessarily sensible goals, and habits and principles handed down by society may at times lead to the best possibility of adaptation. In fact, the hypothetical individual who would be the completely rational man, moved to action only by intelligence, would be a man without qualities and would not have a stable structure of character. Intelligence must be seen as only one function with particular value in relation to organization. It is particularly valuable as a super-ordinate organizing ability and may thus help to handle irrational factors. Freud's words, "Where Id was there Ego shall be," really imply the primacy of intelligence as a regulating factor.

In this context Dr. Hartmann goes on to the consideration of the integrative functions of the ego. The problem of the will, which is more determined by the external world than by instinct, has not been investigated to any degree. We also know much less about the synthetic functions of the conscious and pre-conscious systems than we do about the unconscious. Systems and social values may facilitate social cooperation but at the same time hinder individual adaptation. Science, art and religion are briefly discussed in this connection.

At present a definite theoretical concept of mental health is hard to formulate, not only because social and political factors will prevail but also due to the fact that the process leading to pathology may also lead to adaptation. For the present we will have to limit our theory to the study of relationships, mechanisms of adaptation, synthetic processes and achievement. It is also difficult to define the aims of education, which would include not only adaptation to the present environment and preparations for future environments but an installation of goals and ideas according to the concepts of the present generation. There is a desire to cling to the illusion that knowledge can supply true goals on the basis of objective reality, rather than to admit that it can do no more than to help us recognize our own hierarchy of values. A "natural" value hierarchy valid for all men does not exist and the concept is a screen to make subjective goals and values appear to be objective.

In the study of adaptation the problem of action is important. We call such action realistic when the means toward the goal are at the same time reality-syntonic on a subjective basis and also fit into conditions existing in the external world. The ego uses somatic systems to carry out its actions and well-established achievements become so completely automatized that immediate steps disappear. Similar automatization occurs in perception, thinking and problem solving.

Dr. Hartmann maintains that it is quite legitimate to think of a pre-conscious automatism as a mental apparatus because it has both structure and form. Action implies not only intention, will and motive but also an apparatus to carry out the action. Thus it is of great importance to study these apparatuses, both somatic and mental, which belong to the realm of autonomous ego development and constitute one of the important roots of the ego. The apparatuses, congenital and acquired, which lie outside the instincts at the same time need the instinctual drive as the source of energy. In

recent years the study of the inheritance of mental characteristics has been advanced by the work with twins.

Certain of the ego functions are molded on the pattern of instinctual drives, such as the parallel between giving and taking and projection and introjection. It is quite probable that the apparatuses and instincts arise from a common matrix as do the ego and id. We consider as inborn ego apparatuses only those which are in the service of the ego subsequent to the differentiation of the ego and the id. Although there may be few congenital processes that are apparent at birth, many appear later in the course of maturation as exemplified by the organization and development of the libido. Maturation is a constitutional process furthered by experience and we must assume that they are both mental as well as physical processes. There are various rhythms in the development of inborn processes although at the same time they are affected by environmental factors.

A consideration of autonomous development and the ego apparatus is necessary to understand the individual's relationship to reality and the concept of ego strength. Dr. Hartmann closes with the following sentence. "I will be pleased if you should agree with me that the problems of autonomous ego development, of the structure and rank order of ego functions, of organization, of central regulation, of self-suspension of function, etc., and their relations to the concepts of adaptation and mental health, have a just claim on our attention."

In going over this material, which must be re-read for complete understanding, one comes to the feeling that this work is of major importance in directing our thought into new areas. The aim of psychoanalytic thought has become one of considering the individual in every aspect of his internal functions and his external relationships to the various aspects of life with which he interacts. The emphasis is no longer simply upon psychopathology but upon understanding man in his relationship to both the inner and outer world. In the psychoanalytic, psychiatric or philosophic approach to the study of man one frequently finds the attempt to take an easy path, which would delimit the important factors to either congenital, environmental or social and cultural areas. Here we find an attempt to consider man as the end result of all the complex factors which shape his being. This monograph with its great wisdom will be reread by those few who will find the effort rewarding. It is not for the individual attracted to easy answers and facile generalizations.

A. Russell Anderson